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BRAITHWAITE'S RETROSPECT.

VOL. XLIII. JANUARY—JUNE, 1831.

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THE

RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M.D.,

LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN AT THE LEEDS SCHOOL OF MEDICINE, ETC.

AND

JAMES BRAITHWAITE, M.B., LOND.

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AFFECTIONS OF THE SYSTEM GENERALLY.

FEVERS.—All animal poisons have an *adhesive* attraction for surfaces, especially the walls of a room in which the case has occurred. Hence, in all fever hospitals, and in private houses, after a severe case of typhus has occurred there, it is well to have the walls lime-washed, and those of the hospitals every month or so. (Dr. S. J. Goodfellow, p. 76.)

GOUT.—*Pressure.*—In some cases the pain in the affected joint is really agonizing; yet, though the aggravation of pain is great on any sudden pressure however slight, gentle steady pressure relieves it. Enclose the foot in cotton wadding, so as to make the pressure more easily applied, and more even; then take an elastic bandage, and, beginning at the ankle, gradually apply it round the foot, and, last of all, very gently and evenly round the toe. The relief experienced is remarkable. (Dr. C. B. Garrett, p. 366.)

SCARLATINA.—In all cases of scarlatina, if a moderate but efficient daily action of the bowels is secured, and the action of the skin promoted, we do not run much chance of the supervention of scarlatinal albuminuria. If, however, there is the slightest appearance of the disease assuming a malignant or severe form, immediately give iron in the form of the tincture of the sesquichloride. Along with other tonic treatment it will be found to have a most beneficial action. (Dr. S. J. Goodfellow, p. 75.)

SCROFULA.—*Arseniate of Soda.*—In cases where the diathesis has not as yet produced a cachectic condition, and where the local manifestations are superficial, and confined to the skin, the mucous membranes, and suppurated lymphatic glands, there is no tonic so efficacious and energetic as arseniate of soda. It is an admirable succedaneum of iron, quinine, and cod-liver oil. It ceases to act as a tonic if given in too large doses. The one-fourteenth of a grain may be given at first, according to the age, and increased gradually up to one-fourth of a grain. It may be given in infusion of cinchona as a vehicle. (Dr. Bouchut, p. 15.)

SCURVY.—The main risk in cases of scurvy, as seen in sailors lately landed, is from fatal syncope; hence in the treatment of these cases the recumbent posture must be strictly maintained, and lime-juice must be given to the extent of two or three ounces daily, qualified by a proper amount of sugar. Chlorate of potash will be found of great value in all cases in which the gums are very tender and spongy; and, where there is much irritability, or sleeplessness, opium may be given with advantage. (Dr. S. H. Ward, p. 14.)

AFFECTIONS OF THE NERVOUS SYSTEM.

EPILEPSY.—The author generally commences the treatment of a case of epilepsy by the administration of belladonna in quarter-grain doses twice a day, in pill or mixture. It is a curious observation, that the usual effects of belladonna are rarely produced in cases of epilepsy, or any other convulsive disease (as shown previously in cases of chorea, if we remember right, by Dr. West). In those cases in which there appears to be a tendency to regular recurrence of the fits, give large doses of quinine; five, ten, and even fifteen grains to be given at intervals, shortly before the occurrence of the fit. By this means the postponement of the fit is procured. In cases in which the aura epileptica arises from one of the limbs, a ligature should be worn constantly on the arm, and tightened immediately the aura is felt. When an epileptic patient complains frequently of either a pain or a sensation proceeding from some part of the body, the actual cautery should be applied locally to this part. The effect on the fits is in many cases most marked, their number being reduced considerably. (Dr. Brown-Séquard, p. 39.)

[The treatment of epilepsy by belladonna is insisted on chiefly by Trousseau, who continues the use of the remedy, if necessary, for three or four years. (For his mode of administering it, &c., see *Retrospect*, vol. xxxii, p. 57.) We have ourselves seen a patient, of whom Trousseau publicly asserted that he was confident of curing him by the prolonged use of belladonna; he was, however, uncured at the date when we saw him, and would probably remain so, as the case was one of rotary epilepsy.—EDS.]

NEURALGIA.—*Hypodermic Injection of Morphia.*—The effects of hypodermic use of morphia require to be so carefully watched, and the dose to be so nicely graduated, that, except where the case is very urgent or obstinate, it is much better to apply the morphia endermically (the cutis being first denuded by a blister). One death at least has been known to result from injecting morphia subcutaneously, though here proper care was probably not taken. (Dr. E. H. Sieveking, p. 33.)

An ointment composed of two grains of *veratria* to the ounce of lard will frequently relieve neuralgic pain, if well rubbed into the part. The part feels deadened and a slight rash appears. (Dr. E. H. Sieveking, p. 34.)

PARAPLEGIA.—Cases of paraplegia may be divided into two groups: 1st. Cases in which there is an increased amount of blood in the spinal cord or its membranes. There are always symptoms of irritation, convulsions, cramps, twitchings, erection of the penis, from irritation of motor tract; formication, itching, pricking and other pains, abnormal feelings of cold or heat, of tightness, pressure, &c, from irritation of the sensory tract; diminution of temperature of the paralyzed limbs, wasting of muscles, œdema, bed-sores, alkaline urine, &c., from irritation of the ganglionic fibres. 2nd. Cases unaccompanied by any of these symptoms of irritation. In these, there is a diminished amount of blood in the cord. The former are cases of inflammation or congestion; the latter are cases of white softening, or of reflex paralysis. In the former only those remedies must be used which diminish the amount of blood in the cord; in the latter those remedies which increase it. *Belladonna* causes contraction of the vessels of the cord, and diminution of the amount of blood: so do ergot of rye and mercury, but mercury has such a depressing influence that it is better not to employ it, except in cases of syphilitic paraplegia. Strychnine increases the amount of blood in the cord, and should be used only in cases in which there is no sign of irritation of the cord; ammonia, sulphate of quinine, iron and cod-liver oil, are also very useful in similar cases. Iodide of potassium is one of the most powerful agents in the absorption of effused fluids; it may be employed in either class of cases, but it is especially useful in cases of syphilitic paraplegia. (Dr. Brown-Sequard, p. 26.)

TETANUS.—*Aconite.*—The surgeons of the Middlesex Hospital place their patients, from the first appearance of the tetanic symptoms, under the influence of aconite. At first, three minims of Fleming's tincture are given every three hours, and increased subsequently to five minims, the pulse being carefully watched. At the same time acetate of morphia is injected subcutaneously once or twice a-day. The diet given is liberal, wine and beef-tea as much as can be taken. (Ed. of Lancet, p. 38.)

The *tobacco enema*, half-a-drachm to the half-pint of boiling water, is much praised by Dr. Wood, of Philadelphia. It must be repeated every two or three hours until its relaxing effects are produced. The nervous system is much more readily impressed by tobacco than opium. (Ed. of Lancet, p. 38.)

[We were much struck lately in reading Abercrombie's work on "Diseases of the Stomach" by the statement that he has used the tobacco enema very frequently (in cases of ileus,) but has never seen

any of the dangerous symptoms so often mentioned by authors, and the fear of which have almost excluded it from practice.—Ed. of *Retrospect.*]

WASTING PALSY.—*Galvanism.*—The important thing is, to pass the current through the substance of the affected muscles, and not along the nerves leading to them, nor into the spinal cord. When a muscle wastes owing to paralysis of its nerves, the cause is a lesion of the nutritive operations in the muscle. The two poles of the battery should consist of small brass balls, closely covered with sponges, which may be thoroughly wetted before use. The sponges should not be placed more than an inch-and-a-half apart, and be well pressed in. No sitting should be protracted for more than ten or fifteen minutes, and the affected muscles must be successively stimulated, the operator passing rapidly from muscle to muscle. (Dr. W. Roberts, p. 25.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

LIGATURE OF THE FEMORAL BELOW POUPART'S LIGAMENT.—The doctrine is usually held that the vicinity of a collateral branch, however small, to a ligature, vitiates the process of union, by preventing the effusion of agglutinative lymph. This, however, is not the case: an artery may be tied within a short distance of a large branch on its cardiac side, and if the constitutional health is good there will be no danger of hemorrhage from this cause. One great danger in tying the femoral artery, is wounding the vein; this is much better avoided, if the artery be tied about half-an-inch or or rather more below Poupart's ligament, where both artery and vein are on the same level, and side by side. In this situation the avoidance of the vein is perfectly easy; in the place usually selected it is sometimes impossible to avoid it. It is extremely easy to tie the vessel in this situation, as it lies so very superficially. The incision made must be about an inch and three-quarters in length, *across* the direction of the artery, at the distance of half-an-inch below Poupart's ligament, and exactly parallel to it. (Mr. G. H. Porter, p. 152.)

PALPITATION, WITH PROMINENCE OF EYEBALLS AND GOITRE.—There is a class of cases characterised by violent palpitation, with enlarged thyroid gland, and prominent eyeballs. These cases are rare, but once seen are never forgotten. There is cerebral excitement, and other symptoms of disturbance in the circulation of the brain. This disease is almost exclusively seen in females, and depends on diminution, and not increase, of nervous power. The proper treatment, and one very successful, is the free use of powerful tonics, especially of the astringent kind, as perchloride and phosphate of iron, which act almost as specifics in the disease.

Narcotics may be given at night with great advantage. Highly nutritious food, and pure air are required. Unless aware of the proper treatment for these cases, the practitioner would be most likely to use quite an opposite plan of treatment, and aggravate the symptoms. (Dr. C. H. Jones, Dr. C. J. B. Williams, p. 377.)

PERICARDITIS.—In the treatment of rheumatic pericarditis it is well to consider the local treatment subordinate to that suitable to the primary disease. The best local treatment consists in the application of from four to six leeches, followed by fomentations. If the relief is not complete the application may be repeated. In obstinate cases, blisters may be used. Upon the whole, the disease is one susceptible to a great extent of cure, under mild palliative local remedies and fitting constitutional treatment. (Dr. W. T. Gairdner, p. 42.)

VARICOSE VEINS.—Using a blunt-pointed needle (but still sufficiently sharp to penetrate the skin), pass under the vein a piece of fine iron wire, and let it emerge at the further side of the vein; again introduce it, but so as to pass between the vein and the skin, and let it emerge at the hole at which it was first introduced. Now twist the two ends together, and cut them off about an inch from the skin. The pressure at first must not be such as to cause ulceration of the coats of the vessel, this must only be induced by further twisting the wire, when by previous pressure the walls have adhered from plastic exudation. (Dr. R. J. Levis, p. 229.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

ASPHYXIA FROM DROWNING.—*The Warm Bath.*—The use of the warm bath has been objected to by Edwards, Brown-Sequard, Marshall Hall, and others, but on insufficient grounds. Repeatedly patients take the first sob or inspiration on immersion in the bath. The first thing to be done is to wipe the mouth and nostrils, place the patient on his back with his shoulders raised and supported on a folded article of dress, draw forward the tongue, and inflate the lungs by Dr. Silvester's method, *i.e.*, by raising both arms up by the side of the head, and then extending them gently and steadily forwards for a few moments, so as to make the pectoral muscles draw the ribs outwards; then turn down the arms, and press them gently and firmly for a few moments against the sides of the chest; repeating these movements alternately, deliberately, and perseveringly, fifteen times in a minute. After this the warm bath must be used at once, placing the body in it up to the neck; raise the body in twenty seconds from the water, and dash cold water against the chest. Pass ammonia under the nose. Use again Dr. Silvester's method, or if this be ineffectual, the lungs must be forcibly inflated by an apparatus, or, if one be not at hand, by the mouth of the

surgeon. Remove the body from the bath and rub the surface with dry hot towels. This is the plan followed by the Royal Humane Society, Dr. Marshall Hall's Ready Method having been fully tried, and found to fail. (Dr. Christian, Dr. Sharpey, p. 367.)

ASTHMA.—The best way to administer *stramonium* is to collect the fumes in an inverted glass bowl, from this the patient can readily inhale. The attack is often so violent that the patient cannot smoke a stramonium cigar on account of the urgency of the dyspnœa. (Mr. T. L. Pridham, p. 49.)

PHTHISIS.—*Ozonised Oil.*—If cod-liver oil impregnated with ozone be given in full doses, a remarkable lowering in the rapidity of the pulse will be produced. This is a therapeutic fact of great importance. (Dr. E. Thompson, p. 44.)

TRACHEOTOMY.—The wound in the trachea having been made, frequently great difficulty is experienced in introducing a canula into the opening, especially in children. To effect this, insert the point of a *double* hook through the incision into the wind-pipe, and hold the latter elevated and fixed, then slip up the clasp and allow the halves of the hook to separate by their own elasticity, and so to widen the slit in the trachea; this done, the canula may be passed into the tube with comparative ease, and the hook withdrawn. (Mr. C. F. Maunder, p. 379.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

HEMORRHOIDS.—*Ecraseur.*—It is found that, in a great number of cases operated upon by the écraseur, a large and progressively contracting cicatrix is formed, and, as a consequence, traumatic stricture of the rectum. (Lancet Correspondent, p. 168.)

HERNIA.—*Radical Cure.*—The following is one of the simplest modes of operation, requiring no complicated apparatus, and the results are perfectly satisfactory. All the apparatus required is an elongated body perforated at one end by a hole, a piece of strong thread, and a needle such as is used at post-mortem examinations. The string must be passed through the hole in the end of the body, and the needle, threaded with one end of the string, laid with its concavity resting on the forefinger of the left hand, is then passed up along the cord, within the external ring; the needle must then be turned round, so as to bring its point upwards, and passed with an inclination to the left through the textures, and brought out on the surface of the abdomen; the other end of the thread must then be passed through the parietes in the same way, only that this time the inclination of the needle must be to the right; the two threads must now be pulled tight enough to draw the body up the canal. The piece of tube or wood introduced must

be smeared with cantharides ointment, to irritate the skin, and favour the formation of adhesions. The two ends of the thread must now be tied together, and, to prevent any chance of cutting through the skin too fast, a bit of elastic bougie may be placed below them. For three or four days, or a week, a compress may be placed over the groin, and retained in position by a bandage. (Professor Syme, p. 157.)

The hernia having been returned, and the patient placed in the recumbent position, grasp the sac by the finger and thumb, and slightly raise it; introduce a wire ligature under the skin, and guide it so as to include the greater part, if not the whole of the sac, care being taken not to include the cord. The curved and handled needle used may be so manipulated that the ligature is brought out not far from its point of insertion; by then withdrawing it, and again applying it to the other end, this end may be brought out exactly at the point of insertion, and the two ends may then be tied, if preferred, over a small metal plate. A compress and bandage must then be applied, and the recumbent position of course maintained. (Dr. J. Morton, p. 162.)

A small recent protrusion, which will resist every effort at reduction, can, by a force acting from within, be reduced with the greatest ease. In such cases make a vertical incision an inch or an inch and a-half above the neck of the sac, dividing the skin of the abdomen, and gradually diminishing in extent until the peritoneum is reached, which open so as to admit a blunt hook, or a pair of curved forceps, with which the intestine may be grasped and drawn inwards. Should this proceeding fail, the wound may be enlarged and the usual operation proceeded with. (Dr. J. Niven, p. 166.)

Wutzer's Operation.—There are several causes of failure after Wutzer's operation, which unless known and observed will bring the operation into discredit. It must only be performed in suitable cases, *i. e.*, in strong, otherwise healthy persons up to 40 or 45 years of age, who lead a life of active bodily exercise. If the ring be very large, and the canal much shortened, the operation is useless, except to render a truss effective. If the ring only admit one finger, and the canal be only an inch and a-half long, success is almost certain. In some cases the cylinder has never been within the ring at all; in others, the cylinder has never been properly adapted to the canal, or the pressure has been unequally applied. Sometimes the use of a truss is neglected after the operation, whereas the use of a truss for several months is absolutely essential to permanent success. (Mr. T. S. Wells, p. 158.)

"*The Triple Lever Truss.*"—This truss, which has been recently invented by Mr. Heather Bigg, effectually supports a hernia, having, instead of the objectionable circular metallic spring, a soft-padded

band. This pad contains three small levers, the pressure of which can be exactly regulated by a small button outside. It effects pressure in the same manner as the hand of the surgeon, in an upward, backward, and outward direction. (p. 165.)

LIVER.—*Action of Mercury on.*—It appears now, from experiments carefully performed, and observations carefully made, that mercury is not an hepatic specific at all, and that it is more likely in fact to diminish the amount of bile secreted than to increase it. The root of the entire tissue of errors seems to result from the very evident green stools produced by the administration of calomel; this, however, is owing to subsulphide of mercury, which is of a bright green colour, and may be obtained from those stools by levigation. Calomel relieves some hepatic complaints, but it is by its purgative action, and this may be equally obtained from any other purgative. Dr. Inman, indeed, calculates, from experiments by Kolliker and Scott (Beale's Archives of Medicine), that the chances are six to one that calomel will diminish the hepatic secretion. (Dr. Thudichum, p. 59.)

PROLAPSE OF THE RECTUM.—In most cases, at first, simply the mucous membrane is protruded, but subsequently, if the case grow worse, the muscular coat likewise is protruded. In some cases laxity of the sphincter is the chief cause of the mischief. In the treatment of this affection, when the disease has become very extensive, and it is probable that the muscular coat is protruded as well, and particularly when associated likewise with hemorrhoidal tumours, any operation short of the ligature will be useless in removing the disorder. In the cases so frequently met with in old people, where the prolapse is very voluminous, and the sphincter relaxed, an excellent plan of treatment is, first of all, to apply strong nitric acid on one or more occasions; and, when this agent has had some decided effect, to remove with curved scissors narrow strips of skin and mucous membrane from around the verge of the anus, at right angles to the orifice. These two operations have never been used conjointly in the same case before. The best case for nitric acid, is where the parts are vascular and readily bleed. Where the mucous membrane is thickened it is not of so much use. (Mr. H. Smith, p. 170.)

SALIVARY FISTULA.—Pass a probe, armed with a few threads of silk, from the external opening into the mouth; secure the threads *in situ* for a few days, until a channel is established for the saliva into the mouth. The edges of the external wound must next be pared, brought together, and maintained so by two points of twisted suture, using very fine needles. The wound will heal in a few days. (Mr. R. G. H. Butcher, p. 174.)

AFFECTIONS OF THE BONES AND JOINTS, &c.

AMPUTATION AT THE KNEE-JOINT.—In amputating at the knee-joint (the flap having been made from the calf), it is better in all cases to remove the cartilaginous surface of the femur, as unpleasant symptoms sometimes result from its being attacked by ulceration, and sometimes by death. The best way of removing the cartilage is, to lay the fine blade of a Butcher's saw on the healthy osseous tissue, close to the cartilage, but not infringing upon it, and then cut the bone in a curved manner from before backwards. This form of the bone, free from irritating edges, is better in every way for adaptation to the soft parts. (Mr. R. G. H. Butcher, p. 120.)

CONTRACTION OF THE KNEE-JOINT.—In what cases can forcible flexion and extension be used? When the contraction of the joint is owing to disease which has existed within the joint itself, we we can do little more than endeavour to put the limb into a more favourable position. Where, however, the contraction is owing to thickening *outside* the synovial membrane, we may use forcible flexion and extension with much prospect of restoring the motions of the joint. When inflammation has existed within the joint the synovial cavity is generally destroyed, and motion cannot be restored except to a slight degree, and the employment of much force is injurious. (Mr. H. Coote, Mr. Brodhurst, p. 140.)

DISLOCATIONS.—*Instrument for Reduction of.*—It is a great desideratum to reduce dislocations by some means by which the extending force can be suddenly slackened, so as to admit of the muscles asserting their influence to rectify the displacement. A contrivance by which this can be effected will be found described at p. 150. It was invented by a student at the Aberdeen Infirmary. (Dr. Keith.)

EXCISION OF THE KNEE-JOINT.—To be successful in this important operation observe the following directions. 1. Select the cases judiciously, the bones not being diseased far beyond their articular extremities. 2. The H incision should be preferred, the perpendicular strokes being placed well back, so as to allow all fluids and discharges to drain well away. All portions of diseased synovial membrane must be removed. 3. The patella must be removed, whether diseased or not. 4. All bleeding vessels must be carefully secured. 5. While the patient is yet on the operating table, the limb should be placed in the horizontal position by gentle and steady extension. 6. Great caution must be exercised that the surfaces of the bones may be in contact throughout their extent. 7. The limb should not be disturbed for several days after the operation. 8. In cases where large abscesses form in the vicinity of the excised joint, Chassaignac's drainage tubes may be used with the best hopes of success. 9. Stimulants and sedatives must be freely administered, regulated of course by age, sex, temperament, and habits. (Mr. R. G. H. Butcher, p. 133.)

Immediately the operation is concluded the limb must be forced into the horizontal position, and put up before the patient is removed from the operating table. It must be put up in a solid case, and so retained immovably. When the knee has been contracted for a length of time, it is better to divide the hamstring muscles in preference to shortening the bone, to admit of coaptation, which is difficult in such cases. (Mr. R. G. H. Butcher, p. 129.)

KNEE-JOINT.—*A peculiar disease of.*—There is a peculiar disease of the knee-joint characterised by the following symptoms. There is acute tenderness of the skin, and great aggravation of pain on the least motion, even of the bed on which the patient lies. The joint is swollen, and the swelling not only extends around the joint but around the lower part of the thigh and upper part of the leg; it pits on pressure and a sensation of crackling is felt beneath the finger. The skin is of a pale white glossy appearance. The disease is an inflammation originating in the areolar and fibrous tissues external to the joint, and, Mr. Hawkins believes, in the periosteum covering the lower end of the femur. If the disease is supposed to be gout or common synovial rheumatism, and is treated by colchicum and afterwards hydriodate of potash, the patient will suffer for months from the violence of the pain, and the joint may become disorganized. The best and only successful treatment is rapid mercurialization of the system and local depletion. This is most successful when the disease is early recognised, but even at a more advanced period it will often arrest further mischief. A lotion of cyanide of potassium (4 grains to the ounce), applied tepid, is a powerful local sedative application. (Dr. F. J. Lynch, p. 143.)

LATERAL CURVATURE OF THE SPINE.—It is a common mistake to suppose that, in incipient curvature, the muscles may by proper exercise rectify the impending deformity; whereas, muscular exercise can no more cure a curved spine than it can rectify a knock-knee. Muscular exercise is healthful and useful as an auxiliary, but spinal curvature can only be corrected by mechanical support, and direct pressure. This pressure must be unremitting, and maintained at the proper standard for months or years. The best apparatus is that known as Tamplin's; it is weighty and strong. The pressure is exerted by pads attached to steel rods, moved with cog-wheels. Slighter cases may be cured by the use of stays with lateral supports, or side crutches. These apparatus may be procured at Ferguson's, close to St. Bartholomew's Hospital. (Mr. H. Coote, p. 147,)

OSSEOUS ANCHYLOSIS OF THE ELBOW IN A FAULTY POSITION.—Forcible flexion is out of the question where the union is bony. The best plan is to saw through the lower end of the humerus above the elbow, bend the arm, and a false joint or anchylosis in an improved position is obtained. (Mr. H. Lee, p. 151.)

Scrofulous Disease of the Joints.—Almost every case is curable, unless very extensive disorganization has taken place, if the following treatment can be followed. The patient must reside in the country, in a dry bracing atmosphere, with much exposure in the open air to the light of the sun. There must be a free circulation of the atmosphere around the patient's bed at night, Animal food must be taken daily, also cream and cod-liver oil, and some of the preparations of iron. A healthy condition of the skin must be maintained. These means, along with cheerfulness, mental occupation, encouragement, and suitable local treatment, lead even in most unpromising cases to the best results that can be desired. (Mr. W. Perrie, p. 135.)

AFFECTIONS OF THE URINARY ORGANS.

Albuminuria.—In all cases of acute albuminuria it is advisable to avoid giving elaterium and other drastic purgatives; all stimulating diuretics are contraindicated, the only diuretic admissible being the liquor ammoniæ acetatis. Slight venesection, and warm or vapour baths frequently repeated, are the remedies of most utility. After the acute stage has passed off, and all feverish symptoms have subsided, *tonic* treatment must be adopted, and perhaps no tonic equals the tincture of sesquichloride of iron. When the disease is produced by cold, depletion will be better borne than when produced by scarlatina. When from alcoholic poisoning, general depletion will rarely if ever be called for, but local depletion may be freely resorted to. (Dr. S. J. Goodfellow, p. 79.)

Lithotomy.—Mr. Wood, of King's College, uses a staff (combining the uses of a staff, and of a dilator of the prostate,) the curve of which divides into an anterior and posterior blade. The membranous portion of the urethra being opened by a lunated incision, commencing a little to the right of the raphé, the finger, passed between the blades, dilates the prostate, and enters the bladder. The posterior blade, by means of a lever in the handle, can be made to press downwards, so as to hold the bladder firmly, and prevent its being carried before the finger. A woodcut and a fully detailed account of this instrument is given at p. 175.

Stricture of the Urethra.—*Potassa Fusa.*—The difference between the local effects of potassa fusa and lunar caustic is, that the lunar caustic gives rise to a hard slough, and to the effusion of lymph around it; while the potassa fusa effects a softening and dissolution of the thickened tissue, without producing any surrounding hardness. Hence the value of the latter in stricture of the urethra. (Mr. C. De Morgan, p. 186.)

An instrument has been invented in France, by means of which a stricture may be cut, dilated, and cauterised. No other instru-

ment is required. It consists of a flat metallic tube, seven inches long, to the end of which there is attached a grooved director, forming a slight curve, along the channel of this steel rods are protruded. A woodcut and description will be found at p. 192.

SUGAR IN URINE.—The copper tests are, as is well known, by far the most delicate. The solution known as Barreswil's liquid, forms the most delicate and ready of these tests (a formula for its preparation will be found at p. 91); but for the use of those practitioners who only require it occasionally, the following will do almost as well, and is very easily prepared:—Take five grains of sulphate of copper, ten grains of tartrate of potash, and dissolve in two drachms of liquor potassæ (previously made stronger by the addition of a little extra solid caustic potash—half a drachm will do), by adding this the liquid will keep in an efficient state for a considerable length of time. By the fermentation test, it is impossible to analyse the amount of sugar quantitatively, but by the copper solution this may be effected easily and accurately. (Dr. F. W. Pavy, p. 89.)

URÆMIC NARCOTISM.—*Bloodletting.*—In the treatment of uræmic narcotism coming on suddenly, if the patient is not debilitated by previous disease, nor overloaded with fat, bleed him freely, and at once. The congestion of the kidney and other organs will be removed, besides removing a portion of the poison. The relief is sometimes so instantaneous, that a patient who has lain some time completely unconscious, will be able shortly to converse and transact business. (Dr. Richardson, p. 84.)

URINARY ABSCESS.—Supposing a urinary abscess to point in the perineum, after laying it freely open, do not interfere further. Theoretically, the usual practice of passing a catheter into the bladder seems to be the most appropriate, but practically it will be found that the presence of an instrument is positively mischievous. (Mr. Le Gros Clark, p. 180.)

AFFECTIONS OF THE SKIN, &c.

ABSCESSES, SINUSES, AND ULCERS.—*Effect of Rest.*—There are some cases of abscess and sinus which will not heal, do what you will, unless the parts be kept in a state of perfect rest, and this rest is impossible owing to the constant motion of adjacent muscles, or fascia into which muscles are inserted. In some of these cases the muscle or fascia requires division. In abscess under the scalp, the occipito-frontalis may be kept quiet by strapping the head with long strips of plaster. Similar cases of abscess under the platysma myoides occur frequently, the result of strumous glands; these may be cured by precisely similar treatment, *i.e.*, when they do not heal, as they often do not, from constant action of the platysma myoides. Small facial

abscesses may be cured by applying collodion externally, keeping the aperture in the skin open at the same time; the collodion causes pressure and complete rest to the parts. Many ulcers are exquisitely painful and irritable. This pain will usually be found to exist, especially in one particular point—here a nerve fibril is probably exposed; by dividing a small portion of tissue between this painful spot and the spinal column and close to the spot, the nerve fibrils will be divided, the pain cease, and the ulcer heal. Any granulations, particularly sensitive, may be cut out with a pair of scissors. (Mr. J. Hilton, p. 220.)

How to open Deep Abscesses.—If you plunge the knife into a deep abscess you may cause very dangerous, if not in some cases, fatal hemorrhage. If you wait until the abscess comes nearer to the surface, the patient may die in the meantime. The best plan is to divide the skin and subjacent fascia with a lancet, and then push a grooved probe into the swelling; when the probe has penetrated the abscess, a little pus will show itself in the groove. Now, forcibly push some blunt instrument, as a pair of dressing forceps, along the groove, into the abscess; by withdrawing the forceps with the blades rather open, a lacerated track or canal is left communicating with the abscess. This will not readily heal. For many years Mr. Hilton has not opened a deep abscess in any other way. (Mr. J. Hilton, p. 219.)

ALOPECIA AREATA.—(Syn. *Tinea Decalvans.*)—This disease depends upon the presence of the parasite called *Microsporon Audouini.* It produces patches of baldness, the skin being smooth and very white. First, carefully extract all the hairs within a couple of lines of the patch to prevent the disease spreading, and if any downy hairs are present on the patch they must also be removed. A lotion of bichloride of mercury is one of the best parasiticides which can be used. The growth of the hair may be promoted by the use of gentle vesicants, after the cause of the disease has been removed. (Dr. T. McCall Anderson, p. 212.)

ECZEMA.—Most, if not all, internal remedies are useless in this disease. Arsenic is the only remedy which exerts any influence, and this is only seen in a few cases; it is not worth while trying it except in very obstinate cases. When the eczema arises from the friction of two cutaneous surfaces, the best application is a mixture of starch and oxide of zinc (starch ʒi, zinc ʒiij). In acute eczema, or in chronic, when there is but slight infiltration, and the disease prevails over only a limited extent, the following are good applications: oxide of zinc, ʒj to ℥j of lard; sulphate or acetate of zinc or alum, ʒj to lbj of water; red or white precipitate, 6 to 12 grains to ʒij of lard. When there is considerable infiltration the best application is the sapo viridis, or green soap (a kind of caustic soap used in Germany). This must be thoroughly rubbed in, and then

the surface washed and cold applications be laid on until next rubbing. In many chronic and obstinate cases the only application which suffices, is a solution of caustic potash (ʒj to ʒij of water.) One or two rubbings with this will generally suffice. A mixture of equal parts of tar and cod-liver oil is an excellent application in many cases, but it is not applicable in the early stages of the disease. Cod-liver oil alone sometimes acts well when the skin is dry and scaly. (Professor Hebra, p. 210.)

Favus.—There is an intimate connexion between the strumous habit and the development of favus, hence, in the treatment of this disorder (which is almost confined to the poor and destitute), the adoption of all means calculated to improve the general health is indispensable. Regular exercise, good diet, and the use of cod-liver oil are therefore required. After the scales have been removed, the local application of cod-liver oil, as recommended by Bennett, for the purpose of destroying the favus by excluding it from the air, is attended with very satisfactory results. (Dr. W. Pirrie, jun., p. 203.)

Ingrowing Toe-Nail.—Drop a little hot melted tallow upon the part, and the pain will be at once relieved. (Dr. Clarkson, p. 229.)

Parasitic Diseases of the Skin.—It is almost absolutely necessary for the cure of these affections to remove the hairs by a pair of blunt-pointed forceps, since the parasite is contained abundantly amongst the longitudinal fibres of the hairs, and also that applications may reach the bottoms of the hair sacs. One of the best and most generally used parasiticide applications is a solution of bichloride of mercury (two grains to the ounce of water). Sulphur ointment is more suitable for animal parasites. The mercurial lotion should be rubbed in directly each depilation is completed. In cases of *herpes circinatus* (ringworm of the body), the application of a parasiticide is sufficient, and when the patches cover different parts of the body at the same time, it is best treated by sulphur or mercurial vapour baths. Herpes circinatus, herpes or tinea tonsurans, and sycosis, are only varieties of the same disease. In the two latter depilation is required. Whatever parasiticide is used should be applied immediately the hairs are removed, as the hair sacs are then open. (Dr. T. McCall Anderson, p. 194.)

Prurigo.—This disease occurs only in poor people, who pay little attention to the state of their skin. Any measures which will soften the skin, will remove the papules and itchiness. The best treatment is warm baths, with plenty of soap, and, after each bath, rubbing into the skin an ointment composed of equal parts of cod-liver oil and tar. The itching almost invariably disappears after the third application. (Prof. Hebra, p. 210.)

SCROFULOUS SCARS.—If unsightly puckered scars in the neck be destroyed by potassa fusa, we find on the falling off of the eschar that a smooth even surface is substituted for the previous seamed and corded one. In the course of time, this new tissue regains so natural an appearance as to escape notice. (Mr. Hoffman, p. 189.)

SINUSES.—In cases of subcutaneous sinus, pass a probe along the canal for the distance of an inch, or an inch and a-half, cut down upon its point so as to destroy the continuity of the canal at this spot. Then introduce the probe at the point of incision, and passing it another inch, again cut open the canal, repeating the process until the whole length of the canal is so treated. Pressure must be carefully applied for five or six days, when adhesive inflammation will have obliterated the fistulous track. (Mr. W. Jessop, p. 227.)

[See also "*Abscesses, Sinuses, and Ulcers.*"]

TINEA FAVOSA.—*Carbonate of Copper.*—Carefully clean the head by means of unctuous cataplasms, and, when the crusts have been completely detached, shave it. Then apply an ointment consisting of 2½ drachms of carbonate of copper to 15 ounces of purified lard. This ointment cures porrigo with much rapidity. (M. Huet, p. 209.)

URTICARIA.—Apply locally a mixture of equal parts of liq. plumbi and glycerine. It relieves the painful itching caused by the disease very remarkably. (Dr. Frazer, p. 214.)

WOUNDS.—*Disinfectant.*—A mixture of powdered chlorate of potash and glycerine in the proportion of 2½ drachms of the former to 3 ounces of the latter, possesses a remarkable disinfectant power, when applied to suppurating unhealthy wounds. It forms a very good application to sores of this description. (M. Martinet, p. 367.)

AFFECTIONS OF THE EYE AND EAR.

ARTIFICIAL PUPIL.—*Iriddesis.*—The operation of iriddesis is performed in the following manner. 1. Chloroform is given, the patient being placed in a convenient position. 2. The wire speculum is applied so as to freely expose the eye. 3. A puncture is made, by means of a moderately broad needle, at the junction of the cornea and sclerotic. 4. A loop of fine floss silk, moistened, is laid on the eye over the opening in the cornea. 5. The iris is seized almost midway between the ciliary and pupillary margins by the forceps canula, introduced into the anterior chamber through the puncture in the cornea, and drawn out through the corneal puncture to the requisite extent. 6. The loop of silk is tightened so as to strangulate the portion of iris which has been drawn out. The ends are then cut off, and the operation is completed. The ends must not

be cut off too short, or the knot may be slightly drawn within the wound. This operation is applicable to nearly every variety of case in which an artificial pupil is required. It enables the operator to regulate with accuracy the exact size and position of the pupil he proposes to make, and it secures stability to the artificial pupil, so that it will continue of the size and form it had when first made. The operation is particularly applicable in cases of *central leucoma*, corresponding in size and position to the natural pupil; also in cases of *opaque capsule, with adherent pupil*, in which it is desirable to enlarge the pupil slightly in one direction, so as to expose a clear part of the lens. (Mr. G. Critchett, p. 247.)

Cataract.—The German oculists have now almost completely abandoned reclination, as, even in successful cases, it is found that after a few years blindness returns, owing to choroiditis arising in the situation in which the lens had been placed. The results which are obtained by the ordinary extraction by a flap formed upwards for the hard cataract of old people, and by the linear excision for the soft cataract of young persons, are so satisfactory, that other modes of operation are scarcely necessary. (Correspondent of Medical Times and Gazette, p. 232.)

Congenital Cataract.—Linear Extraction.—Mr. Bowman's method of operating is as follows: Completely dilate the pupil by means of a strong solution of atropine, and, by repeated small strokes of a needle, introduced through the cornea, break up the lens thoroughly in every direction, taking care not to wound the posterior capsule, or enter the vitreous chamber. The lens by this operation becomes thoroughly softened and separated from the capsule. After four or five days, make a linear opening in the margin of the cornea, and with a small spoon or scoop gradually remove the lenticular matter, leaving a clear pupil. Atropine must be freely employed in all the stages of the operation. In cases where time is no object, a more perfect result may be obtained, and inflammation may be more completely avoided, by the older method of solution. (Mr. W. Bowman, Mr. G. Critchett, p. 254.)

Conical Cornea.—There are two indications in the treatment of cases of conical cornea, first, to check the softening process; and second, to diminish the intra-ocular pressure. The former we can only pursue by measures of a tonic kind, the latter Von Graefe has lately proposed to effect by the operation of iridectomy, as employed in cases of glaucoma. The effects of conical cornea may, however, be remedied partially by converting the pupil into a narrow slit. (It is well known that an eye thus affected will see much better through a diaphragm perforated by a small hole, or a narrow slit). The cornea must be punctured with a broad needle, close to its margin, and a short blunt hook introduced, and the pupillary margin of the iris seized, drawn out, and tied by a loop of floss silk

placed on the eye in readiness, exactly as in Mr. Critchett's operation of iriddesis (described fully in this volume). If this does not effect the desired end in about a week, the opposite side of the pupillary margin must be treated in the same manner. Diagrams showing the states of dilatation and contraction in the case of horizontal slit, of vertical slit, and of the balloon-shaped pupil from iriddesis downwards, will be found at page 400. (Mr. W. Bowman, p. 258.)

GLAUCOMA.—*Iridectomy.*—The operation of iridectomy is of no benefit whatever in cases of chronic glaucoma, but, in the acute affection, it is of very great use. In the latter case, we must remember that the disease is one which will not wait, if the proper time is not seized it passes by for ever. (Mr. Dixon, p. 236.)

Some surgeons consider that the smaller the quantity of iris removed the better. This is a mistake; such proceeding is rarely attended with striking success. The segment of the iris must be rather large, and it must be removed completely up to the ciliary margin of the iris. (Correspondent of Med. Times and Gazette, p. 232.)

Scleroto-Corneal Section.—Puncture the sclerotic coat about one-eighth of an inch behind its junction with the cornea with a small cataract knife, cut forwards to about the same extent through the cornea, making altogether an incision about one-third of an inch long, and dividing the outer margin of the iris. A portion of the iris will prolapse, and may be removed; this is rather advantageous than otherwise. The same operation will be of much benefit in rapidly-increasing conical cornea. (Mr. T. Nunneley, p. 243.)

IRIDECTOMY.—*New Operation for.*—Mr. Greenway, of Plymouth, has invented an instrument by means of which an aperture can be punched in the iris, so as, if necessary, to form an artificial pupil in the natural position, and of a circular form. The instrument has the appearance of a syringe, "but, instead of being furnished with an ordinary nozzle, a canula is screwed on to the end of the cylinder, or body, and, there being no piston, the rod, which would otherwise be the piston-rod, is continuous with the blade which lies within the canula. This rod passes through an air-tight fitting at the upper part of the cylinder, and has a spring action. The canula is plano-convex transversely, its bore being one-twelfth of an inch by one-fifth; the plane surface presents near the extremity a circular aperture, about one-eighth of an inch in diameter, for the admission of a portion of the iris; that small portion of the tube beyond this aperture is filled with lead, which serves, not only as a plug to completely obstruct the extremity, but as a point of resistance for the blade. On the convex surface is a small mark which indicates the position of the aperture on the plane surface. The blade is kept

in close contact with the floor or plane surface of the canula by means of a spring, which is interposed between it and the roof or concavity of the canula. Communicating with the upper part of the cylinder is a metal tube, about an inch in length, on which is fixed an India-rubber tube, which is furnished with a mouth-piece at its free end. This may be termed the suction-tube." By means of suction through this tube the iris is seized, and pressure being made on the head of the rod, a circular portion is immediately excised. The instrument, of which an engraving is given, may be procured of Weiss; it is a very ingenious contrivance. (Mr. H. Greenway, p. 233.)

INJURY TO THE EYE BY MOLTEN METAL.—It seems from a case related, that molten metal may be suddenly splashed into the eye, and still very little injury result, the sudden conversion of water into steam protecting the eyes from injury. (Mr. Le Gros Clark, p. 269.)

RESTORATION OF THE PUNCTUM BY A NEW OPERATION.—In cases where either punctum is completely lost, and its restoration is desired, a much more certain plan than any previously followed, is, to pass one of Bowman's canaliculus styles from the pervious punctum into the sac, and out along the lower canaliculus, using a knife to free the point of the style on emerging, or not, as necessary. This proceeding is easier than would be imagined from the acute angle the upper canaliculus makes with the lower at the sac, but the whole course lies within soft parts. The only difficulty arises from spasmodic action of the orbicularis and tensor tarsi, which however may be readily overcome by chloroform. The style used must be slightly bent at the extremity. (Mr. J. F. Streatfeild, p. 567.)

TRICHIASIS.—To completely eradicate an inverted eye-lash, or bundle of them, plunge the point of a needle or fine knife, dipped in caustic potash allowed to deliquesce, into the tarsal margin along the course of the hair or hairs, to the depth of the eighth of an inch. On the second or third day remove the lashes so treated with the forceps; they are pulled out without any difficulty and are blackened at the roots. The bulbs of ciliæ are completely destroyed, never to be reproduced. No inflammation of any moment follows the operation, unless a whole row of ciliæ be removed at a time. (Dr. J. Williams, p. 266.)

MIDWIFERY, AND THE DISEASES OF WOMEN, &c.

AMENORRHŒA.—*Neuralgia.*—Pains under the left breast, in the intercostal spaces, or in the temples, are very frequent and distressing symptoms occurring in cases of amenorrhœa. They are most quickly relieved by anodynes, but generally only temporarily. The use of small doses of arsenic (two drops of Fowler's solution thrice a day) will be of more ultimate benefit, although not so immediately

effectual. When there is any indication of a rheumatic taint of constitution, very satisfactory results will be obtained by the administration of thirty or forty drops of the tincture of *actea racemosa*, or black snake-root (an American plant), three or four times a day, in a little water. The most rapid relief can be given by the subcutaneous injection of morphia. In *chronic and obstinate neuralgic headache*, a very effectual remedy in my hands has been from half a grain to a grain of sulphate or phosphate of nickel. (Prof. Simpson, p. 339.)

APPLICATION OF THE FORCEPS.—The aphorisms of Denman so frequently taught in our medical schools and followed by practitioners, that the forceps must never be applied until the head has rested on the perineum for six hours, or except an ear of the fœtus can be felt, are both bad. There are many cases where such delay would be highly injurious, and it is unnecessary to feel the ear of the fœtus. (Dr. A. B. Granville, p. 409.)

ARTIFICIAL DILATATION OF THE OS UTERI.—When the os is rigid and contracted in the first stage of labour, and turned towards the rectum, instead of downwards and forwards, first influence the patient gently with chloroform in the following manner: Pour a little chloroform into a tea-cup and hold the edge of the cup to the lower lip of the patient, bringing the open vessel a little over the mouth, but not so as to prevent the free ingress of air both into the mouth and nose; the nurse can manage this whilst you watch the effects. Now introduce the forefinger into the rigid os uteri, and, while the patient is under the influence of the chloroform, gently dilate the part by very quietly, during each pain, pulling the anterior lip towards the pubes. You may keep the patient gently influenced by the chloroform till the os has lost all its rigidity and has become dilated. Now leave off the chloroform till the tough perineum has to be dilated, when you may use it a little again. You will thus save hours of misery and do good to your patient. If the chloroform act too much, compound spirit of ammonia either taken or breathed is an almost instantaneous antidote. (Dr. Braithwaite, p. 275.)

CÆSAREAN SECTION.—If the operation be done with resolution and rapidity, no great alarm need result if we find the placenta attached to the anterior part of the uterus. A sudden gush of blood will follow the incision into it, but the operation must be nevertheless proceeded with, and the placenta be extracted first, as rapidly as is consistent with doing it well. (Dr. J. Edmunds, p. 286.)

CANCER OF THE BREAST.—Sulphate of zinc is one of the best tonics which can be given, sometimes the pain is alleviated, and frequently improvement of the general health follows its prolonged use. (Mr. H. George, p. 344.)

Catamenial Acne.—In some persons a crop of acne appears if the proper menstrual flow is at all checked. In many, this eruption disappears if the flow be restored, but in some it still remains persistent. If, in these cases, you apply the oil or butter of antimony with a brush, very thinly, and neutralize it quickly with bicarbonate of soda, the eruption will frequently disappear very speedily; and thus used, the preparation does not cause any pain, nor does it exercise any caustic action. Citrine and mercurial ointments are also very good applications. If one part of the ordinary tincture of iodine be mixed with two parts of the milder aquæ ammoniæ, and allowed to stand forty or fifty hours, the mixture, which is at first brown, becomes quite clear and colourless. As it posseses all the activity of the tincture of iodine, and is yet colourless, it forms one of the best preparations for external application. It may be used with much advantage in cases of acne. (Prof. Simpson, p. 340.)

Chronic Mammary Abscess.—These cases are sometimes very difficult to cure. It has been recommended to pass a seton across them in a perpendicular direction; but milder means may be employed with success. Apply Scott's ointment to the breast, spread on lint, then place tightly over this strips of plaster an inch and a half in breadth, and bandage the whole carefully. (Mr. W. Coulson, p. 337.)

Deficient Involution of the Uterus after Labour.—*Bromide of Potassium.*—Sometimes the uterus does not undergo the normal retrograde metamorphosis after labour, and remains large and heavy, the fibre cells being loaded with fatty particles. The uterus is felt large, soft, and heavy, and there is a feeling of weight and discomfort in the pelvis. In many cases, the abstraction of a small amount of blood from the vaginal portion of the uterus, or the perineum, is called for, and is more especially beneficial in those cases where there lingers any degree of congestion or of inflammatory action in the uterus. The most efficacious of absorbent remedies are the iodide and bromide of potassium. The latter is preferable, however, as it does not produce that marasmus so often caused by prolonged use of the former. The bromide, moreover, acts as a special sedative on the reproductive organs. The dose should be about six grains, three times a day, and it may be continued with iron or other tonics, if anæmia or atony co-exist. Good diet and other hygienic means must be employed. (Prof. Simpson, p. 298.)

Fibrous Tumour of Uterus.—Intra-uterine fibrous tumours may be destroyed by gouging out a portion of their tissue. The vitality of the growth is so low, that the remaining portion gradually disintegrates, and comes away as discharge. As a preliminary step, it is better to incise the os and cervix uteri. The best instruments to use are those of Mr Harper, for which see wood-cuts and description at p. 322. (Mr. I. B. Brown, p. 318.)

INFLATION OF THE LUNGS OF INFANTS.—*New Instrument.*—Dr. Wilson, of Glasgow, has invented an instrument for the purpose of inflating the lungs of infants born in an asphyxiated state. It consists of a small india-rubber ball, to which is attached a slightly-curved german-silver tube about six inches long, and having two openings a short distance from the point, and a larger one almost an inch from the attachment of the tube to the ball. The tube is introduced into the larynx, and the larger opening being closed by the finger, the ball is compressed and air forced into the larynx; the finger must be removed during the expansion of the ball that fresh air may enter by the larger aperture. The insufflation of the lungs must be gently and slowly performed, and the chest may be slightly compressed after each inflation. If there is much lividity, allow a drachm or two of blood to escape from the divided vessels of the cord. The instrument may be obtained from Mr. Hilliard, surgical instrument maker, Glasgow. (Dr. J. G. Wilson, p. 345.)

OVARIAN TUMOURS.—After tapping, apply constant and regular pressure, this much retards the refilling of the cyst. This treatment is only applicable in the monocystic forms of the disease. (Mr. I. B. Brown, p. 326.)

OVARIOTOMY.—If you think ovariotomy advisable in a case, do not delay it, for after the abdomen has been disturbed by repeated tappings, and the constitution drained by the constant refilling of the cyst, the mortality is very much increased. (Mr. I. B. Brown, p. 327.)

When should the clamp be used, and when the ligature, in securing the pedicle? This depends entirely upon the length of the pedicle; if it is pretty long the clamp is much preferable. If short, we are obliged to use the ligature, letting the stump of the pedicle return within the abdomen; when this is done the mortality is much greater. (Mr. I. B. Brown, p. 326.)

Sixteen per cent. of the fatal cases die from bleeding from the pedicle. A most excellent plan, and one which renders the use of the clamp unnecessary, is to cut through the bottom portion of the tumour, instead of the pedicle, leaving a portion about the size of a small hand, attached; this must be retained outside the abdomen. All risk of serious hemorrhage is avoided. (Dr. Tanner, p. 328.)

The metallic sutures or hare-lip pins, used to close the abdominal wound, must be passed through the peritoneal edges, as well as through the integuments and muscle; otherwise the peritoneum will retract, and a raw surface of considerable breadth will be left exposed to the general peritoneal cavity. (Mr. T. S. Wells, p. 329.)

Besides the precaution of fastening the pedicle externally, and making the smallest external incision compatible with the passage of the tumour, other essentials for success are:—1. To pass silver

pins so deeply through the abdominal parietes so as to include the peritoneum. Union by the first intention of the deep parts of the wound is thus secured, and the accidental entrance of any pus into the abdominal cavity is prevented. 2. Nutriment and stimulants must be very freely administered per rectum, when the patient is unwilling or unable to swallow them; they are capable of absorption into the system to a most remarkable extent. (Dr. L. Roberts, p. 330.)

PLACENTA.—*Removal of after Labour.*—It is usual to wait for a pain or to be able to feel the insertion of the cord into the placenta, before attempting to remove it. But pains may mislead, and may arise from other causes than contractions of the uterus, and the feeling for the insertion of the cord is a very questionable proof that the placenta has become detached from the uterus. The great objection, however, is, that frequent and painful examinations are sometimes, and indeed often, necessary. The cord after the birth of the child will be found in a state of flaccidity; in a short time, however, it again becomes turgid with blood, which state again passes into one of flaccidity; and this occurs so regularly that this flaccid state following the turgidity constitutes a sign of detachment, which may almost invariably be relied on. (Mr. J. Clay, p. 291.)

POLYPUS OF THE RECTUM IN CHILDREN.—Our attention is again called to this affection by a case occurring at Guy's Hospital. The polypus had been overlooked completely by several medical men, who evidently had not read the able article written on the subject by Mr. Bryant last year (*Retrospect*, vol. xli., p. 334). This affection is most likely to be mistaken for piles, or simple prolapsus of the rectum. There is always hemorrhage from the bowel. The polypus may be broken off by traction, or removed by ligature. (Mr. T. Bryant, p. 348.)

POSITION IN LABOUR.—In cases of lingering and protracted labour, it is often a good plan to let the woman sit upright upon two chairs fastened to each other *in front* by tape or other strong material, and the *backs* separated one and a-half or two feet as may be necessary. The patient during a pain can take hold of the bed-post, and the accoucheur can from time to time ascertain the progress of the case. The patient must be removed to bed when the head is is nearly born. In all cases in which this proceeding is adopted, the head should be within the pelvic inlet, and the os uteri dilated fully one half; and further, the failure of the pains in the horizontal position must be first evidenced. (Mr. R. Hardy, p. 401.)

PREGNANCY.—*Operations During.*—If it becomes necessary to perform any operation on a pregnant woman, do not hesitate to perform it; but if it can be delayed till the labour is over, defer it.

Some of the most serious operations, as tracheotomy, operations on varicose veins, and removal of condylomata about the anus and vagina, have been performed without any untoward result whatever. (Mr. Shaw, p. 317.)

PREMATURE LABOUR.—*Induction of.*—Pass the tip of the forefinger of the left hand within the os uteri, and slightly dilate it, pulling the neck down a little, and passing the finger round a little inside. This process, if not accomplished easily at first, may be done more successfully upon a second attempt. Upon each occasion some pains will ensue, and ultimately labour will come on, when an elastic tube may be passed up between the walls of the uterus and the membranes, and a little cold water injected. If any resistance is experienced in introducing it, the tube must be withdrawn and reinserted in another direction. Labour will now proceed naturally. (Mr. H. James, p. 282.)

PROLAPSUS ANI OF CHILDEN.—Those surgeons who like to hazard such a mode of treatment, may follow the plan pursued lately in a case of this nature by M. Foucher. This gentleman injected just external to the anus, ten drops of a very weak solution of sulphate of strychnia (twenty centigrammes to twenty grammes of water,) using a Wood's subcutaneous injection syringe. "The cure was immediate." The principle, of course, is the direct action of the salt upon the fibres of the sphincter. (M. Foucher, p. 348.)

PRURIGO PUDENDUM.—In cases of follicular inflammation of the labia, in eczema, and prurigo pudendum, carefully rub over the diseased portions of skin or mucous membrane a piece of cotton-wool soaked in a solution of nitrate of silver. This may be continued two or three minutes at a time, and be repeated every day or two. (Dr. E. J. Tilt, p. 335.)

PUERPERAL INSANITY.—*Its Connexion with Albuminuria.*—It is a curious fact, unknown by the majority of the profession, that in a large proportion of cases of puerperal insanity, albuminuria precedes and attends the first access of the disease. It, however, generally passes off with extreme rapidity, often in two or three days. Hence it has been so rarely found, on examination, that no connexion between albuminuria and puerperal insanity has hitherto been recognised. When the insanity recurs in the form of successive attacks, each attack will be found connected with a fresh appearance of albumen. If the doctrine of Frerichs be right, that the urea retained, as it always is in cases of albuminuria, is not injurious *per se*, but only from its decomposition into carbonate of ammonia, we have some explanation to the phenomenon of one affection (albuminuria with, of course, uræmia) causing two other diverse and separate affections—puerperal convulsions and puerperal

insanity; the former being caused when carbonate of ammonia results from the change in the urea, and puerperal insanity when some other unknown alkaloid product results. (Mr. Calvert has lately shown that various unknown alkaloids are formed during animal decomposition.) (Prof. Simpson, p. 308.)

PUERPERAL MANIA.—If, in the onset of a case of puerperal mania, we can induce sleep, the probability is that the patient will awake up tolerably well, or at any rate, the case becomes much more hopeful. The best remedy is opium, but it must be given in doses of two or three grains. If the patient cannot, or will not swallow, you may succeed in introducing the drug, in the form of a suppository into the rectum. One or two grains of morphia are required when the drug is administered in this form. In some cases sleep may be procured by means of ether or chloroform, and the patient thus anæsthetised has continued to sleep on and has awaked up quite well. (Prof. Simpson, p. 304.)

RETROVERSION OF THE GRAVID UTERUS.—In reducing a retroverted gravid uterus it is very important not to attempt to push directly upwards at first, as the promontory of the sacrum is in the way. The womb must be placed as much as possible in the right oblique diameter of the pelvis, by drawing the os and cervix towards the left acetabulum, and moving the fundus as much as possible towards the right sacro-iliac synchondrosis. It is also well before commencing much manipulation, to introduce compressed sponge into the vagina, to dilate that canal for the more ready introduction of the hand, and to exercise gentle and steady pressure on the uterus. (Dr. T. Skinner, p. 278.)

SORE NIPPLES.—A mixture of equal parts of brandy and glycerine forms an excellent application. (Dr. W. Frazer, p. 344.)

SUPPRESSION OF THE SECRETION OF MILK.—Moisten a flannel with saturated solution of camphor in glycerine, and apply it over the breast. This will often check the secretion of milk very quickly. (Dr. Harriss, p. 344.)

VAGINITIS.—Inject a solution of nitrate of silver (two scruples to the ounce.) This acts equally well whether the vaginitis be the result of uterine catarrh, or occurs spontaneously. It is most conveniently done if a small glass speculum be inserted first; as the speculum is withdrawn the solution follows it and may be received in a cup. (Dr. E. J. Tilt, p. 335.)

VESICO-VAGINAL FISTULA.—Do not operate in these cases when the woman is suckling. Also avoid operating on a ruptured perineum at this period. The patient is more liable to pyæmia at such times. (Mr. I. B. Brown, p. 316.)

Mr. Buxton Hilliard, surgical instrument maker to the Glasgow Royal Infirmary, has invented a set of instruments which we think will prove extremely useful in the tedious operation for the cure of vesico-vaginal fistula. The speculum is one which, preserving the form of the vaginal, expands so as probably to give a better view of the parts than any speculum previously in use. The edges of the fistula are seized by an instrument called a "fistula clamp," which is so contrived that the edge of the fistula, when seized by it, is elevated above the surrounding parts, and only requires shaving off with a slightly curved knife. When Bozeman's knives are used, there is often much uncertainty whether the whole of the edges have been effectually pared or not; when this instrument is employed there can be no doubt about it. The tubular needle invented by Mr. Price is the best instrument for passing the metallic sutures through the edges of the wound. The ends of the ligature are brought together and twisted by means of another instrument, and an oval metallic plate applied, differing from Bozeman's in having nipples attached to it, instead of loose pellets being employed. Much trouble is thus saved, otherwise caused in passing each pellet separately. These instruments will be better understood by referring to the original article and the engravings at page 311.

MISCELLANEA.

CHLORODYNE.—Mr. Davenport's secret remedy, chlorodyne, sometimes produces alarming symptoms, such as complete prostration, distressing cerebral fulness; and forty minims produced complete unconsciousness. It has not greater power over neuralgic complaints than morphia, hydrocyanic acid, or chloroform. (Dr. B. Washbourne, p. 364.)

CHLOROFORM.—*Cause of Death from.*—The cause of death, in some cases, is the position of the patient during the exhibition of the chloroform. In the usual prone position with the face upwards, the tongue falls back, closes the epiglottis, and causes asphyxia, as soon as the insensibility is complete. The lateral or sitting postures are the best during the exhibition of anæsthetics. (Dr. J. Petrie, p. 365.)

HYPODERMIC INOCULATION.—A simple mode of introducing a substance hypodermically is to puncture the part quickly two or three dozen times, the lancet being dipped in the substance used, as morphia, made into a thin paste. (M. Lafargue, p. 378.)

NEW ANÆSTHETIC.—*Turpentine.*—Sprinkle some turpentine on a handkerchief, and let the patient respire the vapour freely. It will be found to allay pain and produce a gentle sleep, and anæsthesia,

not followed by any unpleasant symptoms. It was used with good effect in a case of neuralgia in the course of the supraorbital nerve, and in one or two slight but painful operations. (Mr. J. Wilmshurst, p. 363.)

NITRATE OF AMMONIA AS A TONIC.—In some cases of debility and cachexia the nitrate of ammonia is found to act as a tonic. It is supposed to act by introducing an increased amount of oxygen into the system. It may be given in doses of gr. xv., three times a day. (Dr. W. Coles, p. 362.)

POISONING BY STRYCHNIA.—In poisoning by strychnia one principal cause of death is the diminution of temperature. The patient, must be kept near a good fire, and well covered with blankets. The nausea produced by the administration of emetics is favourable to the recovery of the patient. Copious draughts of warm water must be given, so that the patient may vomit fully and freely. The action maintains and developes animal heat. The violence of the paroxysms may be much mitigated by chloroform, not however given so as to produce complete insensibility. (Dr. J. Part, p. 361.)

THE LAMP BATH.—The following is a very simple and most effectual method of exciting the functions of the skin. Let the patient, in *puris naturalibus*, be seated on a common wooden chair with his feet upon a low stool, the body then enveloped in two or three blankets, the head being excluded, and a large spirit-lamp placed under the seat. In about a quarter of an hour the perspiration streams down the skin. After a time the blankets must be removed, and the patient subjected to a douche of two pailsful of cold water, and then dried with much friction. After which a smart walk may be taken. (Dr. C. Taylor, p. 375.)

A Commentary on Midwifery,

AND THE

DISEASES OF WOMEN AND CHILDREN,

FOR THE LAST HALF-YEAR.

The Diseases of Women have given rise, of late, to perhaps more attention than has been devoted to any other special subject, and have drawn forth papers of the most interesting nature. We have thought it useful, therefore, to make the following Special Report, with such comments as struck us at the time of collecting the information. This report we hope to continue from time to time.

In the management of a Rigid Os Uteri, in the early stages of labour, the greatest care and skill are necessary. We are tempted to dwell on this subject for a few lines on account of the paper of Dr. Charles Arnott, who has mentioned some excellent points, but has omitted others. It often happens that you are called to a case of labour when the pains are severe, but the os is quite undilated, and pointing downwards and *backwards*, while at the same time the head of the child is felt to be pressing forcibly on the anterior portion of the womb, just above the os uteri; in short, the womb is doing its work well enough, but the os uteri is in a wrong direction, and is, as it were, opposing its own dilatation, causing the womb to expend its powers in stretching the anterior parietes, instead of dilating the os. Dr. Arnott practises what most experienced men now do—he carefully draws the os uteri forwards so as to be more downwards and forwards, and so as to be under the influence of the uterine pains; at the same time carefully dilating the os. So far we can agree with him; but when he talks of *notching* the os uteri with a guarded probe-pointed bistoury, although in a *rare minority* of cases, we must be cautious how we approve of such a practice. Certainly Nature herself does sometimes play some odd freaks with the os uteri in such cases, as we have recorded a little further on in the present review; but the cases in which this operation is supposed to be required, must be so rare that we should disapprove of it entirely. When the smallest laceration occurs spontaneously, in such cases the *slit* will almost certainly extend considerably upwards: so long as it remained low enough to include only the parietes of the womb, it would do comparatively little harm, but if it included some of the pelvic fasciæ or peritoneum, or so injured the cellular tissue around it as to cause suppuration

within any of these fasciæ, it would entail endless misery on the patient. We would therefore avoid all notching or slitting up of the os uteri in labour, and trust rather to *chloroform* in relaxing these parts in the first stages of labour; maintaining that in chloroform we have an infallible remedy, the employment of which allows the parts to be dilated either by the finger of the practitioner or the pains of the womb. In our notice of Dr. Arnott's paper we have alluded to the way in which we recommend the chloroform to be used (see page 275.)

To the excellent papers of Dr. Tyler Smith and Dr. Skinner on Retroversion of the Womb, we would next direct the attention of our readers. We differ on some points from those high authorities. In our remarks at page 277, we have expressed the opinion that most writers have erred a little on this subject, and frequently called that retroversion of the womb which is nothing more than the gravid uterus in a slight state of prolapsion, and remaining too long in the lower region of the pelvis during the first months of pregnancy, the os pointing *downwards* and *forwards*, and with the enlarged posterior portion of the fundus still in the hollow of the sacrum. Where the womb is more or less displaced downwards from previous gestations, or from weakness of the parts, how can the fundus be anywhere else in the early months of pregnancy than in the hollow of the sacrum, and more or less pressing on the rectum? This is, as it were, its natural position under the circumstances, and ought not to be called Retroversion or Retroflexion. Neither ought we to be meddling too much with such a case, seeing that the fundus is not prevented from rising above the brim of the pelvis, by being wedged up against the promontory of the sacrum, but simply because it was originally so low in the pelvis that it has not yet, in the usual time, at its usual rate of increase, arrived at this point; but it will, if let alone, soon do so, and meantime, if it be slightly prevented from *clearing the promontory*, it can easily be pushed over this obstruction without all the manipulations so dwelt upon by writers. Take the dried pelvis of a female, and examine its axes in the upright, prone, and supine positions: place this pelvis on the table in its prone position, resting on the promontory of the sacrum and the pubes; now place a good large pear, with its stalk, or a small round soda-water bottle with its neck downwards, and its fundus just *under* the promontory of the sacrum, or rather in the prone position in which you have just placed the dried pelvis, just above the promontory; keeping the neck of the bottle in its position, gently press its fundus with the finger, and observe what a very slight force is required to send it over the promontory, and forwards through the brim of the pelvis, so as to rest on the symphysis pubis. Even so it is easy to reduce the (so called) retroverted womb, by placing the woman *prone* on the edge of the bed, with her legs and thighs on the bed and her head on the floor, but supported by her elbows. As to this being indelicate, such a feeling is not to be con-

sidered when both mother and child are in danger. In this position we shall be able with the greatest ease to reduce any retroverted womb, without introducing the hand either into rectum or vagina, as is recommended by some of our best writers. In such a case there is no danger to the woman till the womb arrive at the promontory of the sacrum, where it may be arrested during the rapid growth of gestation. But how can you reach this point of impaction by inflating the vagina with air, or introducing dried sponge pessaries, and allowing these to expand? The point of difficulty is far above where this kind of pressure can extend. By placing the woman in the prone position on the edge of the bed, as we have described, we can introduce the finger, or a properly proportioned gutta-percha bougie up the rectum, and reach the fundus uteri easily: the least degree of pressure *downwards* and *forwards* in this position will cause the organ to fall downwards and forwards towards the pubes.

Mr. James's way of bringing on Premature Labour is not different in principle from what is often done, viz., dilating the os uteri, separating the membranes round the os, and eventually rupturing the membranes when the time for this has arrived. His practice may be additionally effective in this, that he injects into the womb, between the membranes and the womb, about half-a-pint of cold water, by means of an elastic bottle attached to an elastic tube or male catheter. The addition of the water injection may be good, but we think that the dilating of the os and the separating of the membranes where they can be reached by the finger (which is often impossible), and the rupture of the membranes by a male catheter, will seldom fail, and are so easily done that it is almost a pity that so many fresh recommendations are adopted. It is not an easy matter, in the early months of gestation, to get the finger into the retreating and narrow os and cervix uteri without such a degree of force as is sometimes unwarrantable, especially when a male catheter, either a silver or elastic one, can be easily introduced and accomplish what we want.

It is certainly curious to witness how some of the cases called Spurious Pregnancy resemble true pregnancy, except that no child can be felt by the practitioner. The woman herself will make the strongest assertions that she feels the child distinctly; but we would warn the medical attendant to value no evidence but that of his own finger, or ear, and even his ear may deceive him. Dr. Priestley truly states that, where the distension of the abdomen can be relaxed by chloroform, (as may often be done in these cases of abdominal swelling owing to the muscles of the abdomen) we have a good means of distinguishing them from pregnancy. But sometimes we find that the organic muscles of the intestinal canal, and even in some measure, we have no doubt, the womb itself, are capable of a kind of spontaneous distension, which would not be so amenable to the effect of chloroform. Chloroform, therefore, may deceive us. If the child be of any size, that is, after the fourth month, when the fundus uteri can be felt by

the ends of the fingers appearing above the pubes, we can seldom be deceived by proceeding as follows. The general error committed is in examining the woman, not in the prone but in the lateral or supine position. Now, what we want to do, is to bring the fundus uteri as forwards as possible. Therefore place the woman in the prone position, more or less, either standing at the edge of the bed, but leaning with her face and elbows on the bed, or in bed on her knees and elbows with the face prone. Introduce the forefinger so as to press on the anterior portion of the cervix uteri, and with the fingers of the other hand compress the fundus downwards. You have now the uterus between your two hands, and if there be a womb with anything in it, you will have no difficulty in distinguishing it. Of course this is only *one* of many other ways of distinguishing the true from spurious pregnancy; but where a woman declares positively that she feels the child, when you cannot feel one in this way, be guarded in your diagnosis, for fear of subsequent ridicule.

The common Cranial Perforator, generally used, is a formidable military-looking instrument, although efficacious enough for its purpose. We want, however, a less ugly-looking instrument, and we think that recommended by Dr. Thomas, of New York, will prove to be so. The wood-engraving in article 96 will show the reader what kind of an instrument this is.

The case related by Dr. James Edmunds, at page 286, is instructive. It shows us, First, that in a case of cancerous disease of the os and cervix uteri, where delivery is almost impossible without either destroying the child or slitting up the womb itself, the Cæsarian Section may be safely performed; and Secondly, that even when the placenta is situated in front, and immediately under the incision intended to open the womb, the danger of hemorrhage may be averted by rapidly cutting through the placenta and dragging it out. In the present instance this proceeding stopped the hemorrhage which had commenced violently, and moreover, is an additional proof of the value of Dr. Simpson's practice of separating the placenta in some cases of uterine hemorrhage. We have, however, often been surprised to read of successful cases of delivery where *slitting-up* of the os uteri was obliged to be adopted, and therefore, in such a case as the above, although we admire the judgment and skill of Dr. Edmunds, we must say that, unless some very formidable impediment presented itself, we should have preferred slitting-up the os and cervix and delivering either by embryotomy or turning. In such a case turning would be the most proper proceeding because the most easy, and comparatively safe to both mother and child. No doubt, however, this case was most successful and creditable.

Mr. Clay, of Birmingham, points out an indication by which we can judge of the Separation of the Placenta after the birth of the child, without feeling for the insertion of the cord. When we have ligatured the cord in two places and divided it between the ligatures as usual,

so as to separate the child, if we now examine the maternal portion above the ligature we shall find it flaccid; but if again examined in two or three minutes it will have refilled with blood, and will give a distinct feeling of fluctuation, like a tube filled with fluid; in short, we feel that the vessels have been again filled, although just before they were empty—this shows that the placenta has not yet been separated. But as soon as it does separate, this feeling of fluctuation ceases, and the cord remains flaccid. We have not proved this assertion by our own experience, and it is probable the symptoms will in some cases be doubtful; but if proved to be trustworthy it will be a great addition to our means of accurately judging of this circumstance.

Dr. J. Y. Simpson publishes some excellent remarks on those cases of Enlarged Womb which we meet with after parturition and miscarriages. When the womb remains too much enlarged after labour or miscarriage, and its substance does not undergo the usual process of absorption it is now called *Sub-involution*; when it becomes *too much absorbed* or reduced in size, as it does in some cases, it is called *Super-involution*. It is very interesting to watch the absorption of the muscular fibres of the womb. This is not accomplished by the absorption of the muscles primarily, but the muscle is supposed first to undergo fatty degeneration, and is *melted down*, and absorbed as fat. This fatty metamorphosis commences a few days after delivery, and may be seen as a series of glistening particles in the fibres. The womb may be hypertrophied from many causes as well as from pregnancy; for example, from miscarriages, and from any other cause which *sends* or *detains* too great a quantity of blood in the organ, and which blood is *used up* in its nutrition. Thus a most common source of inconvenience in women is an enlarged womb which is using up more blood than is needed for its normal size and functions. From some such simple beginning we have found all kinds of trouble, such as prolapsions, retroversions, *so-called* (but which are often nothing more than heavy wombs falling back on the rectum on account of their occupying the inferior axis of the pelvis, instead of the upper and middle axes), hemorrhoidal congestions, &c. &c. In these cases Dr. Simpson places confidence in the prolonged exhibition of the bromide of potassium, given in doses of 6, 8, or 10 grains three times a day. He thinks we may continue this medicine for a much longer time than the iodide of potassium, and without any inconvenience. This medicine no doubt is valuable in its place, but what we want in these cases of enlarged womb, is *to diminish the quantity of blood* which has been going to the organ for months and years, and has been regularly *used up*, more or less, in the construction of the hypertrophy, the same as in the arm of a blacksmith from the exercise of his business. We may use iodine and apply leeches to the womb or anus as we like, but we maintain that all remedies are inferior to that of placing the woman, for a considerable portion of each day, on a couch with pillows or a

hair bolster under her pelvis, so as to elevate it and thereby to encourage the veins to empty themselves as much as possible, the same as in hemorrhoids. This must be continued for weeks and months when we have tumours and other enlargements to contend with, for it is just as likely to be the quantity of blood *detained* in the veins of the womb as the quantity *sent* by the arteries, which causes these inconveniences. We think, therefore, that in all these cases sufficient emphasis has not been laid on *position*. It is a general principle in surgery that veins are best emptied when they can be raised above the level of the heart, as in varicosities of the leg. Now if you place a skeleton horizontally on the table in the supine position, it will rest on the end of the sacrum and on the vertebral column, but the vessels in the hollow of the sacrum are below the vena cava and below the vessels of the thighs; in fact, they represent a kind of hollow incurvation in which the blood from the legs is, as it were, enticed to linger before it is forced up the sides of the pelvis to the cava inferior. In order to remedy this encouragement to congestion, we must *elevate the pelvis* several inches above the vertebral column when the patient is *supine*: but the case is quite reversed when the patient is *prone*. Hence, in many pelvic diseases, we prefer to place the patient as much as possible in the prone position, or, when supine, with the pelvis considerably elevated. We think these suggestions will improve the treatment both of the sub-involution and super-involution cases, so well described by Dr. Simpson. The super-involution cases are just the reverse of the others: here the womb is absorbed too much. This kind of case, however, is comparatively rare.

It is sometimes difficult to account for the sudden way in which women die after labour. This difficulty is attempted to be explained by calling it Puerperal Fever. For our own part, we consider that it is often nothing more than the *inoculation* of the woman by the hand of the practitioner. Hence we have long held the opinion, in which we are supported by some of our best writers, that the practice of Midwifery ought to be as much as possible separated from the practice of Surgery. It is wrong in any one going from some bad surgical case or *post-mortem* examination, in which his fingers have been bedaubed by some highly objectionable matter, immediately to the bedside of a woman, to assist her in labour. We have it authenticated by numerous instances, that when such a thing is done, the greatest danger to the woman will result, and frequently death itself. You may say that in such a case, when the parts are bloody and covered with secretion, that absorption of poisonous matter will not occur—but we know that it does occur. Observe the common process of vaccination: You make a small incision or a scratch in the arm sometimes it bleeds a good deal, but nevertheless you insert the vaccine matter, which in fact is well mixed with the blood. But what occurs? The vaccine poison is carefully selected by the absorbents

from the blood, and a genuine vesicle is formed, notwithstanding the bleeding; the hemorrhage in short makes no difference. In some measure the inoculation of a woman by the finger of the surgeon who has just arrived from some bad surgical case is similar. Puerperal fever is the result, and the woman dies. We maintain, therefore, that obstetric practitioners ought as much as possible to avoid surgical cases. These remarks are confirmed by the late discoveries of Dr. B. W. Richardson, of London, respecting clots of blood and fibrinous deposits in the veins and right side of the heart, which we think will account for many of those rapidly fatal cases called puerperal fever, and which we have not been able satisfactorily to explain. We have hitherto been satisfied by saying "the woman was struck down by a poison." Let us see if we can throw some light on these cases. Take the one related by Mr. Walford. This case indeed occurred a considerable period after labour, but it only illustrates what in other cases occurs earlier. The lining of the uterus was vascular; "some of the uterine veins were blocked up with fibrinous clots, and this state could be traced into the body of the uterus: a coagulum existed in the right uterine vein close to its entrance into the vena cava." "The right side of the heart was filled with clotty blood, and a fibrinous clot extended from the auricle into the ventricle, and in the right ventricle was a layer of fibrin of a rose colour. The tricuspid valve was a little thickened, and one of the larger divisions of the pulmonary artery was completely blocked." In this case we think it easy to trace the mischief from the womb itself. Imagine one of these fibrinous clots in one of the large uterine veins which possess no valves, and which communicate so rapidly with the vena cava; it becomes loosened and detached, and must almost of necessity be washed along by the venous current into the cava and right side of the heart. It is now almost generally acknowledged that these coagula are very liable to follow uterine phlebitis: and, in a woman under these circumstances, the whole blood is poisoned and much more liable to the separation of its fibrin. We are supported in these opinions by the first authorities, and we again urge that the practice of surgery and midwifery ought to be as *much as possible* separated from each other. This we know cannot be done completely, but "to be forewarned is to be forearmed." The discoveries of Dr. Richardson will throw more light on this subject, and enable us to diagnose as the cause of death some sudden and violent interruption to the action of the heart, in consequence of fibrinous clots washed into it from some other region, in some cases probably caused by a poison acting on the veins, as in the uterus; and in other cases, independent of the specific and prostrating influence of poison thus applied. (See Mr. Crisp's paper, p. 46.)

In the treatment of Puerperal Mania, we have frequently great control over the case by attention to the following things. Instead of bleeding and using other depressing agents, when the pulse is

strong at the commencement of the case, we can reduce both its power and frequency by judiciously giving the tincture of aconite, or the tincture of veratrum viride. Both these medicines have a powerful effect in reducing the circulation, and must be carefully watched: with care they will reduce the pulse beautifully, but must not be continued longer than necessary. Dr. Simpson calls our attention to these medicines, and also to his practice of giving opiates by the rectum. He says you will often find it impossible to give an opiate by the mouth, but you can always introduce a suppository of two grains of opium, or one grain of morphia, into the rectum. He relates a case in which he introduced two grains of morphia. Dr. Simpson has drawn our attention frequently to articles of the Materia Medica which are new to us. There seem to be many powerful remedies in the American Materia Medica which we have not used much up to the present time, but which are gradually obtaining notoriety in Great Britain. For example we have not used the tincture of black snake-root, the *Actea* or *Cimicifuga racemosa*. When the patient has been a good deal pulled down, and is mentally depressed, with a loss of energy and memory, we often give her good diet, wine, tonics, &c., and perhaps an opiate at night. In these cases the tincture of actea will be useful. The Americans use it in acute rheumatic fever, which is often thus cut short by the eighth day. Dr. Voris, of Rochelle, New York, thinks that it is quite an antidote to rheumatism. A strong tincture is made of the root, and doses of 30 or 40 drops are given every few hours. It is also used in chorea and other nervous disorders, but Dr. Simpson has used it with good effect in puerperal hypochondriasis and depression, in doses of fifty drops three times a day. The account which he gives of its effects in one of his cases is highly interesting.

It is now well understood that Puerperal Convulsions are constantly preceded by Albuminuria; so is also Puerperal Mania, but perhaps not so commonly as in the case of convulsions. Dr. Simpson remarks that, in puerperal insanity, if we examine the urine before the attack we shall probably find albumen. Soon after the attack of insanity the albumen often disappears. "The fire of disease goes on burning in these cases after the lighted match is merely applied, and the strange morbid clockwork runs on, as it were, after the key that wound it up is withdrawn." It would be a useful rule, therefore, in all suspicious cases of pregnancy to examine early for albumen. How many catastrophes might we not thus avert, and what useful warnings might we not give both to mothers and friends. It is singular that these cases more frequently happen in first pregnancies. When the nervous system is thus poisoned by the decomposition of urea, whether in the form of carbonate of ammonia or not, it is like a Leyden jar highly charged with electricity, and ready to go off on the first opportunity; especially on receiving any sudden shock, from the officious kindness of friends, such as evil, or even good, news. Hence, when

we know that in any given case albumen is in the urine, we can offer timely warning to our patients and her friends. This practice is too much neglected by accoucheurs, perhaps because it has not been thought of.

Much has lately been written on Vesico-Vaginal Fistulæ, and various instruments have been devised for the performance of this difficult and tedious operation, but we have seen none to surpass in simplicity, and apparently in effectiveness, those figured at pages 313—316, invented by Mr. Hilliard, instrument maker, Glasgow. This gentleman seems to have witnessed several of these operations, and observed the defective instruments used. The reader will be interested with his account. We are warned by Mr. Baker Brown not to operate for vesico-vaginal fistula when the woman is suckling. He relates a case which was apparently unsuccessful from this circumstance.

When a patient applies to you in a state of anæmia from repeated floodings, you naturally examine in expectation of finding a polypus. In many cases, however, there is not a polypus, so called, with a peduncle attaching it to the uterus, but a regular Fibrous Growth, with a broad attachment to the womb. In such a case it is necessary to *incise the os uteri*, so as to get at the tumour, and then to devitalise it, so to speak, by doing it considerable injury; the tumour then sloughs away to a considerable degree and preserves the woman from her repeated hemorrhages. Even simply incising the os uteri will often stop the hemorrhage without doing anything else, we suppose somewhat on the principle that, in cases of placenta previa, to separate this body by means of the finger from the movements and dilatations of the os, the severe flooding of such cases is often arrested. Mr. Baker Brown not only incises or slits the os uteri, but *gouges* pieces out of the tumour or breaks it up with scissors. It will, afterwards, be likely to come away in lumps with offensive discharges. We suspect that in some of the unsuccessful cases the peritoneum may have been wounded, or the discharges may not have had free vent downwards, so that purulent accumulation may have occurred. It will be found that you may even wound the peritoneum, if the orifice be at the most dependent part, so that accumulations may find an easy exit. In one of Mr. Brown's cases the womb was retroverted, and the tumour could not be reached from the os; he therefore made an incision in the posterior wall of the womb, which was very thin, and carefully carried the incision up to the os and towards the fundus, when the fibrous mass could be seen and felt. It was then freely *gouged* three times, at intervals of a few days. The result is not reported, but was expected to be favourable. At page 323 the reader will find Mr. Harper's instruments represented, for what he calls *coring* these tumours. It seems to us that the great danger in these operations will be in implicating the peritoneum. You may see a womb injured a good deal at its os and cervix, but as soon as the injury reaches the anterior or posterior prolongation of the peritoneum, it becomes highly dangerous. These

cases show that we may do a good deal towards getting rid of tumours of the womb by opening into them from the outside of the womb, instead of from the inside, *provided we keep low down and avoid the peritoneum*. A case which illustrates in some measure these remarks is published by Dr. Herbert Barker, in which the os and cervix uteri were torn off by the child's head, and came away entire, the child escaping from the uterine cavity without passing through the os uteri at all. The os even remained undilated, and the woman did well.

Another way of removing Fibrous Growths is by what is called the process of *Enucleation*. But in these cases you can get at the substance from *within* the womb; in those related by Mr. Brown you can only do it from *outside*. Dr. Davis gives us a very good instance of enucleation and removal of a fibrous tumour of the womb. He succeeded first in detaching or enucleating the tumour from below upwards, so as to separate a good portion from the surface of the womb. By this process he seems to have made it resemble a pedunculated polypus, and then proceeded to detach it more completely by dragging down the mass so as to get it partly into the vagina. He carefully examined that he was not at the same time inverting the uterus, which, by the by, we should not be so afraid of doing in such a case, as the displacement would not be difficult to rectify. By a combination of traction and twisting of the tumour close to its adhesion he succeeded in removing it. These cases show what ill-usage the womb will bear without injury to the woman; and this remark is exemplified in other cases mentioned in the Obstetrical Transactions, where the womb was severely used without any ill consequences resulting. But the apparent violence in these cases of fibrous tumours and polypi is necessary, and, in the hands of an experienced man it may be done safely. Tumours in the womb, such as polypi, may be dragged down carefully, so that the finger, or the scissors or scalpel, may be used with safety.

An interesting discussion took place at one of the meetings of the Obstetrical Society in London, in February last, respecting Ovariotomy. Considerable difference of opinion seems to exist amongst those who have practised this operation in London. The points of difference, may, at first sight, seem trivial, but in fact they are very important: and we think that these apparently little things are not sufficiently noticed. Dr. Tyler Smith operates by making a small abdominal incision, tapping the cyst, and bringing it through the wound. The peduncle is well secured by a ligature, and this ligature is *allowed to fall into the pelvis*. Dr. Smith states that the only objection to thus allowing the ligature to fall into the pelvis is the length of time before it comes away. In the paper as reported in the Lancet, we found no mention made respecting the *kind* of ligature; we should consider that to return a common ligature into the pelvis, where it will remain for a considerable period, causing inflammation of a more or less suppurative character, to be objectionable. If the metallic ligature could be used it would be less objectionable. The common ligature

causes *suppurative*, the wire ligature causes *adhesive* inflammation. And when we know that the peduncle of these cysts is often composed of a substance of a very low degree of vitality, we can account for the long delay of the ligature in coming away, and can thus perceive its danger. It will sometimes be 20 or 30 days in coming away. It seems to us, therefore, the best to bring the peduncle *as near as convenient* to the external wound and then secure it. Mr. Wells strongly objects to the ligature being returned into the pelvis, and secures the peduncle as near to the abdominal wound as possible, objecting to Mr. Brown's use of the clamp, except as a temporary measure while he secures the peduncle by ligature. Mr. Brown, on the other hand, prefers to secure the peduncle at the abdominal orifice by his *clamp*, and when this is too heavy, he uses a pair of common *carpenter's callipers*. But only when he can secure the peduncle *easily* in this situation; when it is too short to be thus easily secured he uses the ligature, like other operators. We must confess that this seems to us an excellent proceeding. If you can easily secure the peduncle and cause its adhesion to the abdominal parietes, it seems best to do so; but in cases of difficulty and length of peduncle you can readily have recourse to the ligature. Injecting the cysts with iodine is useless when the cysts are compound, and confidence is nearly lost in the iodine even when there is only one cyst Those cases succeed best which have not been previously tapped repeatedly; repeated tappings disturb the parts, and injure the constitutional powers. It is interesting amidst all these operative proceedings to watch Nature in her own way of doing things. The surgeon taps the cyst from *above*, but Nature does not always do this; she often taps the cyst from *below*. We have read of several instances in which the cyst burst downwards, and a case is related in this volume in which the cyst burst into the rectum. After repeated tappings, and twice injecting with iodine without relief, the patient was sitting on the close-stool and felt "something hard pressing into the rectum." This was followed by a *snap* and gush of fluid; twenty-seven pints of thick fluid escaped per anum. Now, when we remember how easily we can feel the enlarged ovaria in the female through the rectum, we can suppose that in this case a fluctuating tumour might have been felt, which might have warranted its puncture. A large portion of the rectum, the lower parts of the womb and the bladder are, we all know, uncovered by peritoneum in the pelvis, and, curious enough, the more they are enlarged the larger is the portion below uncovered by peritoneum. This would almost seem to invite the operating surgeon to look for a vent for the fluid *downwards* instead of above. We would recommend surgeons to study more carefully the pelvic fasciæ, and especially those points where the pelvic viscera may be punctured without wounding the peritoneum. Speaking, however, of the peritoneum, Dr. Simpson says in one of his lectures, partly published in this volume, that a probe may be pushed through the fundus uteri and peritoneum

without danger. His words are, "although the sound may doubtless be passed into the peritoneal cavity with impunity, yet it is not an experiment one would willingly and wittingly repeat." We mention this simply to show that wounds of the peritoneum are not necessarily fatal, but must in some measure depend on whether the wound be made in such a position that all foreign matter may easily escape, and not collect in its cavity. If we could find a secure road into these ovarian cysts, from the rectum or vagina, we cannot but think that it would be an improvement on the present way of treating these cases. We remember a very interesting paper by Mr. Bainbrigge, of Liverpool, in *Retrospect*, vol. xv, p. 372, in which this able surgeon alludes to various modes of curing these ovarian cysts, and amongst others he mentions how the cyst or cysts burst spontaneously through different parts, some through the abdominal walls at the umbilicus, some through an opening into the vagina, one into the rectum and vagina, one into the bladder, and one into the intestinal canal. He refers the reader to these several cases in the different journals, and states that many more such might be found in which Nature herself was the surgeon. Even in our present volume at p. 332, a case is related by Mr. Fox, of Weymouth, in which the ovarian cyst evidently burst into the bladder, and thus was spontaneously cured. Now that it is discovered that the fine exploring needle, or fine trochar and canula can be passed on their journey of discovery into most important parts, without injury, we may safely predicate that the time will come when these and other obstinate pelvic diseases will be subjected to operations from below rather than from above. The operation of ovariotomy seems to become more frequent and more successful. Its success depends on attention to many little things; at least these things appear trivial, but they are in fact most important. For example, these cases do better when the external wound is closed by passing *metallic sutures* or *hare-lip pins through the peritoneal edges of the wound*. One important property of the peritoneum, like other serous membranes, is to unite rapidly by adhesive inflammation, and we have often wondered why hospital surgeons did not take advantage of this in healing wounds of the intestines. Instead of carelessly returning a wounded intestine into the abdomen, or even allowing a mucous structure to be in contact with a serous structure, or, perhaps, in contact with cellular tissue or fat; if they would carefully unite by fine metallic sutures two wounded serous membranes, they would probably adhere rapidly. Dr. Tyler Smith says that in a case of ovariotomy, where the peritoneal surfaces had been carefully approximated by removing the metallic suture through the adjacent softer tissue, the serous membrane united, and arrested the suppurative inflammation which took place in the whole length of the wound in the abdominal walls. Had the peritoneum not thus arrested the advance of the suppuration, death must have ensued; whereas

the patient recovered. Mr. Wells says that the metallic sutures may be passed through the peritoneal edges with safety, and adhesion will take place in three days, when they may be removed. This is a simple principle of surgery, but not hitherto sufficiently noticed nor carried out in treating wounded viscera covered by serous membrane.

For the Resuscitation of Still-born Infants our instruments are not good. A catheter, male or female, or any other such contrivance introduced into the trachea, or the mouth of the practitioner applied to that of the child, with other practices, have been resorted to, to inflate the child's lungs. One of the simplest or readiest methods is what we occasionally adopt, viz., covering the child's mouth with a fold or two of clean linen, pressing the larynx on the œsophagus, so as to prevent the air passing down it, and then applying the mouth to the mouth of the child and gently blowing down the trachea, taking care to inflate very gently, for fear of rupturing the tender air-cells. Then compress the chest so as to force out the air. The simple fact of forcing air *out* is even more important than forcing our own breath *in*, as it creates a kind of vacuum, which will be filled by the natural expansion of the parietes of the chest drawing fresh air in, even without any further effort of the practitioner. This operation, however, we think may be improved by adopting the suggestion of Dr. J. G. Wilson, of Glasgow, who uses a vulcanized india-rubber ball with a silver tube attached to it. This silver tube is introduced into the trachea like a catheter, and the ball is then compressed with the palm of the hand, air being thus forced into the trachea. A small hole in the tube, within reach of the thumb, can be closed and opened at pleasure, so as to supply fresh air. Of course, you may supply any quantity of air, with this little instrument; it may therefore be useful to resuscitate drowned persons, and ought to be possessed by every medical practitioner.

The usual Obstetric Position in labour is not always the best in a tedious case where there is obstruction, or where the pains fail in power. Although the womb may be called an organic and involuntary muscle, yet it is influenced by the voluntary nerves, and may be assisted, like the rectum in defæcation, by the muscles of volition This is often best done in the upright position. We call the attention of the profession to a paper by Mr. Hardy, of Hull, in the Obstetrical Transactions, which we give at page 401. He shows that in difficult and tedious cases it is very useful to apply a suitable bandage to the abdomen, and place the woman between two chairs, a little separated, in the upright position: thus one side of the woman will be on one chair, and the other side on the other chair, her thighs partly supporting her as well. In this position she can often *bear down* with more force, and the weight of the child may likewise assist a little by the simple power of gravitation. This reminds us that many years ago we perused a paper respecting the Chinese mode of delivery, which was by the woman sitting on the broad edge of a kind

of tub, thus leaving the perineum and other parts perfectly at liberty to be stretched, and which position did actually facilitate the labour. Indeed we can imagine that, in cases where the delivery had to be accomplished without any assistance from the medical attendant, this position would be the best. It would, however, leave the woman unassisted in cases of flooding, and where there was any difficulty with the placenta.

The treatment of Menorrhagia depends on so many things, that it is often difficult; but the result is so seldom dangerous that we cannot agree with the extreme measures which are sometimes adopted to relieve it,—such as first dilating the os and cervix uteri, and then scraping off the internal membrane and injecting tincture of iodine. When the flooding is caused by one or more polypi in the womb, it is proper to dilate and get rid of them; but this can be done with comparative ease by dilating the parts and using the finger, or a pair of very small, but long curved scissors. These scissors, however, should be *curved at a right angle* (so to speak), and only for about one-eighth of an inch; the curve should indeed represent more than a semicircle, so that when introduced they will not catch hold on any contiguous structure, and when brought out again their points can be protected by the finger, or even by placing a small bit of cork upon them, or, if preferred, the points can be blunted. A large polypus can thus be slowly but surely divided by scissors of this description. Remember that when you introduce curved scissors of the usual description into a narrow os uteri, and then attempt to cut through the thick peduncle of a polypus, you dont know exactly in what direction you are cutting, but with a pair of scissors such as we describe, and with the forefinger of the left hand on the peduncle, you may cut away with safety; you know that you are cutting *towards* your finger, and not into the parietes of the womb itself. When the flooding is not owing to these polypi, or when you have extracted them, we need not *scrape off* the mucous membrane of the womb to cure the hemorrhage. If you like, you can *wipe out* the organ with weak nitrate of silver solution; but we seldom fail in checking the discharge, if *very severe*, by the common plug, made with a long bit of rag, well greased and fitted to the os uteri, and following this up with our various styptics, such as tannin, gallic acid, and the tinctura pinûs laricis, in half-drachm doses, every hour or two.

When a woman does not recover well after labour, and where some obscure or severe pain exists in the pelvis, look out for an abscess in consequence of Pelvic Cellulitis: examine both rectum and vagina carefully. The pus will frequently make its way on the front or back of the vagina, and even burst into the vagina itself. To the practised finger it will not be difficult, in this case, to find out the seat of mischief, or rather, where nature wishes for relief. If you are doubtful in such a case, you need not be afraid of using one of the finest exploring needles, as recommended by Dr. Simpson. This fine needle may be

pushed into much more vital parts than the parietes of the vagina without incurring danger. A confirmation of these remarks will be found in Dr. Gibb's paper on pelvic cellulitis.

We must point attention to the recommendation of Assalini's Forceps, by Dr. Granville, and slightly modified by him, which serves the purposes of both the long and the short forceps. It is handy, easy of application, and requires no *cross-locking* of the blades. These are certainly great advantages in some cases, but not in others.

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ART. 1.—ON THE IDENTITY OF THE TYPHOID AND TYPHUS FORMS OF FEVER.

By Dr. JOSEPH BELL, Physician to the Glasgow Royal Infirmary.

[It is at once conceded by the author, that the symptoms of the two affections are not precisely similar; but he contends that the points of dissimilarity are neither so constant, nor so well defined, nor so important as to authorise the conclusion that the two forms of disease arise from different specific causes. Again, the symptoms which are identical are much more numerous and important than those which are non-identical. In another part he asserts that the dissimilarities which do exist are merely differences in degree, arising in consequence of differences of constitutional vigour, or the amount or virulence of the poisonous agency from which the diseases arise.]

Dr. Bartlett has, indeed, supplied the true explanation, when he states "that the blood when drawn from the body has its proportion of fibrine diminished in a degree closely corresponding to the gravity of the affection;" and that in typhus "it is of a dark colour, and non-fibrinous." This fact was pointed out long ago by Andral, and affords a most satisfactory explanation of all the dissimilarities which exist in the symptoms. For example, when the blood is not much impaired, the eruption will chiefly consist of congested papillæ, or at most of the effusion of a little liquor sanguinis, which may have its fibrine consolidated, forming a little conical spot. But when the blood has been nearly defibrinated the eruption will be darker, softer, and more difficult to efface, as fluid blood has been effused. I need not pursue the subject further, because on the slightest reflection it must become obvious that constitutional causes, combined with differences in the amount or virulence of the exciting poison, must influence the symptoms. These facts afford a most satisfactory explanation of the differences which are found to exist between the two forms of fever. So long as the advocates of the non-identity theory accept of this principle as explanatory of the dissimilarities seen in epidemic diseases, they are bound to show how it will not apply to typhus and typhoid fever. This they have never attempted. Again, they ignore one of the best-established principles in medicine, namely,

that poisonous and medicinal substances even in the same doses produce dissimilarities in their effects on different patients; and that differences in the amount of the dose is followed by different effects in the same person. These are medical axioms—indeed, they may be denominated self-evident propositions, and are in daily use to explain the differences which we perceive to exist in the phonomena of disease and the results of remedies. Strange to say, the advocates of the non-identity theory keep these facts entirely out of sight in the question at issue, and rest their conclusions on an entirely opposite foundation, namely, that dissimilarity in symptoms proves a difference in the nature of the existing cause.

Again, let us refer to syphilis—how many different phenomena have we resulting from this poison? In one patient ulcerated tonsils; in another lepra; in a third impetigo; in a fourth iritis; in a fifth nodes; in a sixth rupia; in a seventh psoriasis; in an eighth lupus, and so on. Surely these dissimiliarities are much more striking than those seen in fever, and yet no one doubts of the identity of their origin. Examine the effects of opium or alcohol on different persons in the same or in different doses, and we have the most startling dissimilarities in the phenomena produced. It is, therefore, a most obvious fallacy to argue that a mere dissimilarity in the symptoms of diseases proves the non-identity of the cause. In the question at issue, the differences are so slight (indeed, often so much so that none can be defined by the most acute and careful observer), that it is almost incomprehensible how any person possessed of the most ordinary intelligence, could for a moment entertain the doctrine of the non-identity of their origin. This becomes still more extraordinary when we reflect on the fact that some of the symptoms which are said to be so dissimilar often become convertible. Thus, for example, the eruption of scattered rose-spots occasionally becomes mulberry in colour and extensively diffused. This I have often witnessed. I have seen a patient labouring under a well-marked attack of typhoid, after a severe epistaxis have the rose-coloured elevated spots converted into dark-coloured non-elevated maculæ, at the same time a large additional number making their appearance, so that in the course of a few hours a case of undoubted typhoid eruption became converted into an unmistakable one of typhus. I have seen the same circumstances occur when the patient's strength became reduced from other causes, such as a severe purge, much undue exertion, &c. Dr. Kennedy of Dublin has lately drawn attention to this circumstance, and it affords me much pleasure to corroborate his observations. The theory of the co-existence of the two poisons, so ingeniously adduced by Dr. Murchison of London, will not explain away this important fact.

We therefore find, in the first place, that the differences which exist in the symptoms of the two forms of fever are merely those of degree. 2nd, That these dissimilarities are not uniform. 3rd, That the symptoms most dissimilar are convertible into each other. The

inference is consequently inevitable, that such differences do not authorize the conclusion that the two forms of disease are non-identical in their nature. The differences which exist denote nothing beyond mere varieties in the manifestation of the effects of the exciting cause—varieties arising from peculiarities of constitution and other concomitant circumstances.

I have examined the bodies of *fifty-one patients* who have died from fever—ten of the typhoid, and forty-one of the typhus form—with the following results:—

1. In five who died before the 8th day, more or less congestion or inflammation of the mucous membrane of the small intestines, and deposit in the aggregated follicles were detected.

2. In eighteen who died after the 8th, but before the 15th day, inflammation of the mucous membrane, softening and ulceration of the aggregated follicles, and enlargement of mesenteric glands were observed.

3. In twelve who died after the 15th, but before the 28th day, there was seen extensive chronic ulceration of the mucous coat—in one of these, perforation and fatal peritonitis.

4. In six who died after the 15th, but before the 28th day, death arising from cerebral and pulmonary disease, complete cicatrization had taken place in four instances, and nearly so in the other two; a few small ulcerations still remained.

5. In six who died after the 28th day, extensive thick-edged ulcers existed in the ileum of all the cases, also in the cœcum of two, and in one case the colon was extensively ulcerated.

6. In four who died after the 28th day, complete cicatrization was detected; two of these patients died from cerebral disease, one from extensive bad sores and cellulitis, and another sunk from erysipelas.

Twelve of these inspections were made whilst I resided in the country; six among private patients during the epidemic of 1846–7: and twenty-one were made in the Clyde Street Fever Hospital, under the charge of the late Mr. Jas. Thomson; six were made during the years 1852–53, at the Royal Infirmary; one last year, in private (at Partick); the remaining five are those narrated in the number of this Journal for last January.

I beg to remark, that in all the cases, placed under the 2nd, 3rd, 4th, and 5th heads, cicatrization of numerous follicles was detected. In many it was evident that only some of the glands had degenerated into chronic ulcers. I have never examined the intestines of a fever patient who died from the effects of intestinal ulceration, without observing numerous cicatrices of primary ulcers. This fact has been altogether overlooked by the advocates of the non-identity theory; I beg most respectfully to direct their attention to the matter in their future inspections.

Taking into consideration the results of my own observation of

fever, I am forced to the conclusion that disease of the mucous membrane of the small intestines constitutes the anatomical lesion in typhus as well as in typhoid fever, consequently the two diseases are identical in this essential pathological lesion.

I have read very carefully the works of Louis, Chomel, and of numerous other French writers on fever, and I have been unable to discover a single important point in which the disease which they describe differs from the fever that I have witnessed. Be it observed, that neither Louis nor Chomel contend that the fever of France is different from the fevers of this country. On the contrary, the latter undoubtedly regards the fevers of London, Dublin, Edinburgh, and Glasgow, as narrated by Tweedie, Elliotson, Christison, Marsh, Stokes, and Miller, as identical with the disease which he saw in Paris. In discussing the causes of fever in reference to contagion, he compares the fevers described by the above-named writers with those which prevailed in France, and dwells upon the similarity of their etiology, symptomatology, and pathology; and besides this he always designates the British fever by the term "typhoide," "l'affection typhoide."

It is of great moment to keep in mind that Dr. Williams and many other writers have divided fever into two forms—typhus mitior and typhus gravior; the former corresponding to the first three varieties of Chomel, and the latter with his ataxique and adynamique. It is also worthy of remark that Dr. Williams employs the terms typhoid and typhus synonymously, and it is evident from his work that he was quite conversant with the writings of Louis and Chomel, and it is equally obvious that he regarded the disease which they described as identical in cause, symptoms, and pathology with the disease he witnessed in London—the result of the post-mortem examination in his cases corresponding in every particular with the anatomical lesions found by the French writers in their experience.

We have seen that the result of Dr. Bright's dissections of fever cases exactly coincided also with that of the French physicians. The same remarks will apply to the experience of Dr. Armstrong. I hold that these unquestionable facts furnish the very strongest argument in favour of the identity of the fevers of London and Paris.

It may be objected, in the first place, that Williams may have been influenced in his views by the works of Louis and Chomel, but this objection will not apply either to Bright or Armstrong, as they both had recorded their observations prior to the publication of the opinion of Louis. With regard to Dr. Williams, it is merely necessary to point to the circumstance that, so far as the question at issue is concerned, it is not his opinions that are of importance, but the *facts* which he records regarding the symptoms and anatomical lesions which he witnessed. These are perfectly identical in every particular with those narrated by Louis. But it may be objected, in the second place, that Drs. Williams, Bright, and Armstrong confounded typhus and typhoid cases together—that they were not aware of the distinctions which

have been more recently expiscated by Stewart, Jenner, and others. To this objection it is a sufficient reply, to remark that the description which Bright, Armstrong, and Williams give of the symptoms of the disease, perfectly accords with the disease described by Louis as typhoid. A reference to the cases narrated by these authors will satisfactorily establish the correctness of this statement.

Secondly, all the inspections made by these three distinguished physicians revealed the same anatomical lesions that have been described by Louis. It must, therefore, be admitted that, so far as the post-mortem experience of Bright, Williams, and Armstrong is concerned, all the cases which they inspected were typhoid. Now, it is the very climax of absurdity to suppose that they could possibly have inspected only the typhoid cases, and overlooked those of the typhus form. Hence we perceive the utter worthlessness of the objection urged by some of the most ardent advocates of the non-identity theory, with the view of invalidating the legitimate conclusions deducible from the observations and experience of Bright, Williams, and other writers on the character of the fevers of this country.

Again, it must be conceded on the one hand that Armstrong and Bright describe a disease which would be defined by the advocates of non-identity as typhus, and on the other it cannot be denied but that disease of the mucous membrane was detected on the inspection of the bodies of those who died from the disease. Farther, in the epidemic of typhus described by M. Reveille Parisse the glands of Peyer were found inflamed. Forget and Cruveilhier found the same diseased condition constantly present in the inspections which they made during the epidemic of typhus in 1814. Pinel reports the same circumstance as occurring in his experience. Hildenbrand also asserts that he found the glands of Peyer always diseased in typhus. Herzog tells us that in the exanthematous typhus which prevailed in the duchy of Posen in 1829-30, he found inflammatory conditions of the mucous membrane of the intestines present, Peyer's patches being tumefied, livid, and with surrounding congestion. Fleury and Pellicot, in their account of the typhus which prevailed at Toulon in 1832, inform us that the inflammatory condition of the intestinal canal was slight and variable, and that no ulceration of Peyer's glands was detected.

Let us next turn to the experience of physicians to whom the doctrine of non-identity was familiar. I beg to direct attention, in the first place to the experience of the late Dr. Perry of this city. In a paper published in the *Edinburgh Medical and Surgical Journal* for January, 1836, he sums up the results of his carefnl observation (extending to upwards of four thousand cases and three hundred microscopic inspections) in the form of sixteen propositions; in the last of these he states "that dothinenteritis, or enlargement of the mucous follicles of the smaller intestines and enlargement and ulceration of the aggregated glands of the lower third of the ileum, *occur in*

combination with contagious typhus, and are to be met with in about *one in six* of those who die *from typhus.*"

I would like very much to know how the advocates of the non-identity theory would attempt to set aside this evidence.

Dr. Stokes of Dublin is the next author to whom I beg to refer. The accuracy of his powers of observation, the extent of his experience, are too well known to permit of any allegations against his ability to discriminate between the symptoms which characterize the two forms of fever. He observes that:—

"Many circumstances observable in the epidemic fevers of Ireland, must make us cautious in drawing too strong a distinction between, not only the maculated and the non-maculated cases of fever, but also between typhus—properly so called—and the typhoid disease already specified. It is not to be denied that the characteristic cases of either of these groups of diseases are different; and we have seen that with respect to the condition of the heart, which is the subject more immediately in hand, some remarkable contrasts have been found; yet we cannot help believing that these diseases are but varieties; and that they are results of the same poison or exciting cause acting on individuals in different states; but we need not here enter into lengthened discussions on this point. Let it suffice to indicate generally the grounds of this opinion; they are as follows:—

"1. That epidemics of fever may occur in which, with well marked petechiæ, the follicular ulcerations of the intestine exist in a large proportion of cases.

"2. That in cases of the best marked maculated typhus we have found occasionally the above anatomical condition, and this at a time when no epidemic disposition to the disease could be supposed to exist.

"3. That we have frequently observed, in cases where a large number of a single family have been successively attacked by contagious fever, that every form of the disease, from the most malignant typhus to the mildest typhoid fever, may be presented by different members of the family.

"Lastly, that a patient who has gone through the typhoid fever in its best marked character may, after an apyrexial period of a few days, be attacked with the most severe form of maculated fever; and conversely, that in certain cases where the first fever was maculated typhus, the symptoms in the second attack or relapse were those of typhoid fever.

"These circumstances, when fairly considered, makes a strong case against the existence in these countries of two essentially different forms of fever; and, if anything was wanting to strengthen this conclusion, it is the great fact that although in the so-called typhoid fevers we are not so often nor so urgently requested to employ stimulating treatment, the general principles of management in both forms of disease are truly the same."

Let me next turn the attention of the reader to the work of Dr. Magnus Huss of Stockholm. This physician, after twelve years' experience of fever in the Seraphim Hospital of Stockholm, visited the principal hospitals on the Continent, America, &c. After giving a very lucid exposition of the points of similarity and dissimilarity which he has observed to exist between the two forms of fever, he states his opinion to be "that typhus and typhoid fever (typhus petechialis et abdominalis), such as they appear in the north, neither can nor ought to be *considered as two distinct* maladies. but only as two varieties of the same morbid procees; and he adds, "I am persuaded such are the opinions of the majority of Swedish physicians." This may consequently be considered as the opinion of the north on the subject.

Be it observed, that the grounds on which this opinion has been founded are fully given by the author in his introductory chapter. In this chapter the reader will find numerous most important observations bearing on the subject. both as regards the disease in its epidemic and sporadic attacks. But I must refer to the work itself for further information on the subject.

Skoda is the only other author from whom I will at present quote. He thus states the opinion which prevails among the physicians of Germany regarding the identity of typhus and tpyhoid fevers. "We consider that the disease here is *one*, and ought to be called abdominal typhus; that the typhoid fever of the French is not to be distinguished from what they call typhus; that exanthematic typhus does not constitute a specific affection; and that it is nothing else than the union of their typhus with the accidental appearance of different cutaneous exanthemata."

The preceding facts, collected from writers of all countries, and from men who wrote both prior and subsequent to the promulgation of the doctrine of non-identity, completely coincide with the result of my experience on the subject, namely, that in typhus fever the *same condition of Peyer's glands exist as were found in typhoid.*

I beg briefly to repeat the facts of the case. Williams, Armstrong, and Bright, of London, have found ulceration and disease of Peyer's glands in typhus. Parisse, Forget, Cruveilheir, Pinel, Hildenbrand, Herzog, &c., have found the same ulceration in their inspection of patients dying from typhus. Dr. Perry of Glasgow (an advocate of non-identity) found *ulceration* of Peyer's gland in the proportion of about 20 per cent. out of upwards of 300 inspections of *genuine typhus.* Dr. Stokes tells us that he has found disease of Peyer's glands in the best marked cases of genuine muculated typhus. Huss states that both he and his brethren of the North of Europe have found the same morbid condition present in genuine typhus. Skoda tells us that he and other German physicians have found the same occurrence in undoubted typhus. I have placed on record in this journal *five* well observed cases of *typhus*, in which the same morbid lesion existed. I

repeat that these *five* cases are fair specimens of many others in which the same conditions were found.

I hold, therefore, that these undoubted facts, surely justify the conclusion that typhus and typhoid fever are identical as to the pathological conditions of the small intestines; or, in other words, they are identical as to anatomical lesion.

[The writer considers that if it can be proved that the same phenomena accompany the two forms of fever during life, and that the two affections can be traced to a common origin, that then it must be granted that a "complete" proof of their identity has been furnished. Dr. Bell has seen numerous instances of both forms of fever occurring in the same family, and at the same time.]

A young farmer, aged 25 years, was seized with the typhoid form; he had a few elevated, rose-coloured spots on abdomen and chest; his tongue was chopped and red. About the end of the second week, diarrhœa and other abdominal symptoms supervened; these conditions existed during the space of two weeks, after which he slowly recovered. At the end of the fourth week of the illness his father, aged 58 years, was attacked with febrile symptoms. On the sixth day of the disease an extensive typhus eruption made its appearance on chest, abdomen, and extremities, along with other symptoms of typhus, such as stupor and depression; he required the free use of stimulants, but had neither diarrhœa nor any other abdominal symptom. On the fourteenth day improvement commenced, and his recovery was rapid. During convalescence his daughter, aged 22 years, was seized with fever; on the seventh day her whole body was covered with the dark mulberry rash peculiar to the typhus form; she died on the eleventh day. On the day of her funeral, another brother, aged 30 years, was attacked; the disease assumed the typhoid form, under which he lingered upwards of a month, having suffered severely from diarrhœa. This family lived in an elevated situation about a mile from the village of Barrhead It was considered that the son who was first attacked, caught the infection from a labourer who had recently recovered from fever.

On another occasion I attended a boy, aged 14 years, during an attack of well-marked typhoid; he suffered much from diarrhœa; became greatly emaciated; his illness lasted upwards of one month. He had three sisters, aged respectively 17, 22, and 24 years; two brothers, the one aged 19, and the other 26. His father was aged 59, and his mother 53. The two youngest sisters and the youngest brother were attacked, though not simultaneously, yet very nearly so (in the course of the third week of the lad's illness), with well-marked typhoid symptoms, viz. scattered rose-coloured papular eruption, diarrhœa, &c. Next week the eldest sister and brother were seized with the disease in the genuine typhus form, and had extensive mulberry eruptions, stupor, &c. The father was next attacked with

typhus; the mother was ultimately seized, and died on the ninth day having presented true typhus symptoms. The daughter, aged 22, also died, but from extensive ulceration of the ileum. The others recovered. Ten weeks elapsed from the first seizure till the death of the mother. The family lived in an elevated situation in one of the cottages attached to Ferneze spinning-mill.

I might quote from my note-books many similar instances, showing, that in the same family at the same time, and in the same bed, I have had some members labouring under well-marked typhoid, and others presenting equally distinctive typhus symptoms.

In glancing over some of these old dusty pages, my eye has fallen upon a memorandum of a very striking instance of this community of origin. In the year 1841, a young woman came from Glasgow to Barrhead; she obtained lodgings in one of the worst ranges of buildings in the village, the houses being both greatly overcrowded and badly ventilated. I was called to see her on the third day after her arrival; she had all the symptoms of fever. I immediately communicated with the Inspector of Poor, and recommended her removal to the Glasgow Royal Infirmary, being convinced, that if she were allowed to remain, the disease would spread extensively among the inhabitants.

In these days parochial relief was administered on exceedingly economical principles. The inspector very kindly visited the patient, but he did not participate in my apprehensions; however, after a little importunity on my part, he granted a line of admission to the Infirmary, but would allow half-a-crown only to pay a cart to convey her there (a distance of seven miles). Of course no person would accept of such an insufficient remuneration, consequently she was allowed to remain. The case turned out typhoid; on the ninth day a few rose-coloured spots (about a dozen) appeared over chest and abdomen; about the twelfth day diarrhœa accompanied with pain and gurgling on pressure in the right iliac fossa. The illness lasted nearly six weeks. Mark the consequences—a girl, aged 20 years, a factory-worker (who lodged in the same room), was seized about the beginning of the third week; a few days later the mistress of the house was attacked, one with typhoid, and the other with typhus. Next, a young man who lived in the opposite room was seized, then his sister, and next his father; the children had the typhoid, but the father the typhus form. The disease went on spreading, until no fewer than thirty cases occurred in this range of buildings, some presenting typhoid and others typhus symptoms. Let it be observed that there were was not a single case of fever in the locality when the girl arrived from Glasgow. After the disease had existed some weeks in the buildings to which she came, it next spread, not only to other parts of the villiage, but extended to Neilston. So much on the one hand, for the effects of parochial economy, and on the other, for that

which is of more importance, our immediate object, viz., the community of the origin of the two forms of fever.

In Glasgow I have not had the same opportunities of tracing cases of fever as I had in the country; but in the wards of the Royal Infirmary, I have seen on several occasions some members of a family labouring under typhoid, and others under typhus. For example, five members of a family were admitted in August, 1853; three had the disease in the typhoid, and two in the typhus form. Again, on the 23rd July, 1859, a girl, aged 23, was admitted on the 5th day of fever; on the 7th day the mulberry eruption of typhus covered almost the entire surface of her body. Convalescence commenced on the 2nd of August. and she was dismissed on the 16th. A cousin who lived in the same house with her, was seized with febrile symptoms on the 23rd, that is, seven days after the girl returned home; he was admitted on the 30th; he had a well-marked attack of typhoid, a few rose-coloured spots, which appeared in successive crops, and abdominal tenderness and diarrhœa. He was a month under treatment.

Another girl, aged 19 years, was admitted on the 4th of August, labouring under fever of nine days' duration; her body was covered with a very distinct mulberry rash; bowels constipated. The other symptoms, which occurred in the course of her illness were distinctly typhus. Her two brothers, aged respectively 7 and 14 years, were admitted on the 10th and 14th of August, both having the disease in the typhoid form.

The few cases to which I have now alluded by no means exhaust my experience on the subject—an experience which is far from being unique, or unprecedented in the practice of other physicians. In the last number of this journal I quoted the result of Dr. Stokes' observations on this point. He tells us "that we have frequently observed in cases where a large number of a single family have been successively attacked by contagious fever, that every form of the disease, from the most malignant typhus to the mildest typhoid fever, may be presented by different members of the family.

[The writer then proceeds to consider the objection of those who consider the two diseases non-identical—that these cases are so rare as to be of no moment. Huss speaks of them as of frequent occurrence, and Stokes says that he has frequently observed them. One of Dr. Jenner's arguments is drawn from the exanthemata.]

The proper argument furnished from a consideration of the history of exanthematous diseases, is the very opposite of that deduced by Dr. Jenner. We see that a poison essentially the same produces in different individuals non-identical symptoms, and different pathological lesions, and therefore the differences which we see between cases of fever should be considered as mere dissimilarities in the manifestations of the same poison or specific cause. It is as absurd as it is illogical to argue from the fact that the specific poison of one exanthematous

disease will not produce the peculiar manifestation of another, that, therefore, typhus and typhoid fever must each have its own specific cause. The diseases, as viewed in the argument by Dr. Jenner, are not parallel; and, besides this, there does not exist the slightest logical connection between the premises and the conclusion.

His second argument is equally fallacious; it is founded on the circumstance that during three years, viz., 1847, 1848, 1849, he ascertained among the patients admitted into the London fever hospital, that only two instances occurred in which typhus and typhoid cases came from the same house and family. He affirms that in every other case the typhoid and typhus patients came from different localities. He therefore infers that the two diseases must arise from different specific poisons.

The fallacy of this argument is very obvious. In the 1st place it is a well known fact that during the prevalence of any epidemic, the cases which occur among the destitute and overcrowded localities are much more malignant than those which appear in other places of the same district. 2nd. The period of three years is by far too limited to authorize the general, indeed, I may say the universal conclusion deduced by Dr. Jenner. 3rd. The two exceptions upset his conclusion. These two cases are *positive* facts, and are of immensely greater value in reference to the question at issue than many negative instances.—*Glasgow Med. Journal, July and Oct.*, 1860, *pp.* 173, 305.

2.—ON ENTERIC FEVER.

By Dr. JOHN RICHARD WARDELL, Tonbridge Wells,

[Of course our readers are aware that, by those who have had most opportunity of judging, fevers are divided into typhus, enteric or typhoid, relapsing, and febricula. The two first are the principal and of most importance. Dr. Wardell published some papers on this subject fourteen years ago, founded on the observation of 1200 cases of fever in the hospitals of Edinburgh. At that time he believed enteric and typhus fever to be the same.]

Enteric fever begins more insidiously than typhus, and much more so than the relapsing. In the latter the attack is sometimes quite sudden; a person may go to bed comparatively well, and in the morning be seized with shivering, headache, pains in the limbs, and other symptoms. There is less dulness and stupidness in the enteric than in typhus, in which the great nervous centres seem prone to be more readily impressed, as if the poison at once affected animal life. It is quite true, however, that all forms of continued fever in the symptomatology of accession present much in common, and, under an experienced eye, it is often difficult during the first few days, or even until the rash appears, to arrive at a decided diagnosis. In typhus, the muscular system evinces great prostration, not only at the first,

but throughout the disease; as a rule, the patient lies helplessly on his back.

Dr. Jenner has noticed that in the enteric type patients have a greater tendency to "get out of bed" than in typhus; the delirium is of a more vivacious kind; they are less easily restrained. In many cases the delirium does not come on until the second or third week; when it does in the first week, it is to be looked upon unfavourably. In typhus, it supervenes before the end of the first week, when it increases, and in fatal cases ends in coma. In relapsing fever, there is comparatively little delirium and head complication. On reference to my own account of that disease, in one table giving particulars of 450 cases, leeches were applied to the head, on the average, to 1 in 6·62; in another set of 80 cases it was a predominating symptom in the small proportion of 1 in 11·42. According to Louis, the brain and its membranes, of those dying of the enteric, rarely present any such marked appearance as might be deemed of potential consequence entering into the causation of death. To those who have had considerable experience in morbid anatomy, it is well known that the encephalic mass gives little explanation of the essential nature of continued fevers. A small amount of sub-arachnoid and ventricular effusion, a pinkish flush of the cortical substance, or it may be a few puncta in the centrum ovale, constitute often the entirety of morbid phenomena, and I have repeatedly examined this organ when it evinced no lesion. Even coma may not be traced after death. There is no doubt that the views enunciated by the late Dr. Clutterbuck and those professing similar opinions—regarding fever to consist of inflammation of the brain—led to much mischief in practice, and that a more correct pathology (which is, it cannot be denied, an approach to the once widely received and then discarded humoral doctrine) has been succeeded by a more successful mode of treatment—viz., that treatment which as far as possible dispenses with depletive measures, which is more expectant and conservative in its aims, and which, by having a just relation to a set of morbid actions not primarily located in any single organ or separate tissue, but pervading the entire organism, opposes asthenia. The late Dr. Alison, whose clinical assistant I was, and whose practice I carefully observed, used to dwell with much emphasis upon the Cullenian maxim of "averting the tendency to death;" and although he did not carry the wine and brandy remedy to the extreme which by some it has latterly been advocated, yet he had long seen, with much sagacity, that acute diseases, and more especially fevers, had, from some subtle and inexplicable cause, become less sthenic in their nature, demanding remedies which in previous years were accounted inapplicable or decidedly prejudicial.

Those organs and surfaces which are of an eliminative, a depurative, or defecating character—those emunctories and outlets whereby the products of organic waste and effete matters are carried off, are precisely those which are most liable to become affected in the progress

of fever. That poison which by a most rapid multiple has so vastly increased as to contaminate the whole of the circulating fluid, in obedience to certain vital laws inherent in the organism, by such channels becomes expelled, hence, according to the power of such agent, the excess or defect of function of such organs and surfaces; and thus it is that the skin, the mucous membranes, and the parenchyma of secernent viscera more especially manifest the effects of the specific poison. Ancient theory and every-day facts convince us of those conservative qualities which the system possesses, whereby it essays to eject what it cannot assimilate, and to get rid of such noxious matter as it may have contracted. In the exanthemata, the skin and the mucous membranes of the air-passages are the chief seats of its determinations; in enteric fever, the pustulation is in the digestive tube, mainly in the ileum; in typhus, the mulberry rash evinces an effort made at the superficies to throw off the poison; and in relapsing fever, the powerful diaphoresis, which so frequently at once resolves the fever, inculcates the same doctrine. It now being conceded that the three eruptions peculiar to the respective types of continued fever, are as pathognomonic of those varieties as are the eruptions in the exanthemata, properly so called, it would really seem that it is merely an arbitrary distinction on the part of systematizers and compilers of nosology, whereby they are differently arranged. They all have many features in common; they all originate from peculiar morbid poisons, requiring a greater or less period of incubation; they all pass through a certain train of febrile phenomena; and in all the great centres of animal and organic life are in varied degree affected. Again, there are many points of resemblance in the complications which arise, in the demand of treatment, in the sequelæ, and in the phenomena of the fatal issue. If we were to make a hasty comparison between small-pox and spotted typhus, how many characteristics they manifest of a like nature, still unquestionably caused by poisons, with which like only produces like. Both originate from peculiar poisons, are contagious, require a certain period for development, are diseases in which the prime agent specially operates upon and multiplies in the blood; in one the eruption may be looked for on the third, in the other on the sixth day; in both are all the pyrexial symptoms, as a quick circulation, high temperature, furred tongue, anorexia, diminished secretions, and a great impress is made on the nervous system; in each organic complications of the inflammatory kind may arise; there is delirium and often coma. In the one form, death mostly takes place on or about the eleventh day; in the other, rarely till after the twentieth; and both may have sequelæ attacking the same organs and tissues. And similar comparisons might be drawn between any two types taken from continued fevers and the exanthems. Although human reasoning cannot say why it is that in one variety the pustulation is in the intestines, in another in the skin, yet accumulated observations have long shown, that between the skin and the mucous membrane of the digestive

tract there is a peculiar sympathy, and that between the enteric and variolous phenomena many of the fundamental symptoms exemplify no slight or casual features of resemblance.

Dr. Kennedy, of Dublin, in a paper read a few months ago before the Medical and Chirurgical Society, propounds the doctrine that typhus and typhoid (enteric) are mere varieties originating from the same poison. Dr. Jenner's facts are opposed to this opinion. Of relapsing and typhus I can speak with much certainty. Sixteen years ago I maintained from very elaborate data their distinct essence, and such doctrine still holds good. In more than 1200 cases I never saw typhus and relapsing blended. The infection caught from one fever never produced the other. Like always produced like in a multitude of instances. I have given in my papers thirty-two cases, in which the two forms succeeded each other within a short space of time. Seventeen out of the thirty-two who had passed through the relapsing, contracted typhus during convalescence, or within the brief period of three months. The proofs of the non-identity of their essential cause were as clear as the common-sense proofs we have, and as practice ever tells us, of the non-identity of small-pox and scarlet fever. If typhus differs from the relapsing, why may it not differ from the enteric?—*Lancet, Nov.* 17, 1860, *p.* 482.

3.—*General and Clinical Remarks on Scurvy.* By Dr. STEPHEN H. WARD, Physician to the Seamen's Hospital, Dreadnought.—The treatment of scurvy may be considered under the heads of *curative* and *preventive.* The following is an outline of the plan of curative treatment adopted at the *Dreadnought:*—From two to three ounces of lime-juice, qualified by a proper amount of sugar, are given daily; oranges occasionally when in season; milk and beef-tea, where the state of gums is such as to render solid food inadmissible; otherwise the ordinary diet, consisting of potatoes, vegetable soup, beef and bread. In all severe cases, and in slighter cases where there is marked anæmia, the recumbent position is strictly maintained. This is, indeed, the point of treatment of most immediate moment; the main risk being from fatal syncope. induced by suddenly putting upon the heart more work than it can accomplish. Proper diet and rest are alone perfectly adequate to the cure of the worst forms of the disease. Something must also be set to the account of the moral influence of the sense of security imparted to the minds of the poor sailors on getting into hospital under proper *régime*, and feeling that the exciting and aggravating causes of the malady no longer exist. Special medicine is not, however, quite powerless in the treatment of scurvy. Mr. Corner, when resident medical officer, reflecting upon the service rendered by chlorate of potash in other forms of mouth affection, was led to infer that it might be equally beneficial in the scorbutic disease of the gums. He accordingly tested cases in which it was administered,

against others, of equal severity, in which it was not, with the result of decided testimony to the efficacy of the drug in facilitating the cure of the mouth affection. A positive advantage, moreover, is gained by the rapid cure of the gums, in enabling us to put the patients earlier upon the more nutritive solid diet. Another drug, opium, is also useful at times for the relief of sleeplessness and irritability. I should have remarked that the chlorate of potash did not seem to have any influence upon the other symptoms of scurvy. Doubting the soundness of the premises upon which their administration was based, and satisfied with their failure in the hands of others, I have not cared to make trial of other salts of potash in this disease, to the exclusion of lime-juice or its analogue. The certainty with which the physician can predict the recovery of the very worst cases of scurvy, even those in which life is all but extinct, is very satisfactory, inspiring confidence in himself, and enabling him to impart it to the patient.—*Lancet, Dec.* 8, 1860, *p.* 557.

4.—ON THE ARSENIATE OF SODA IN SCROFULA.

By Dr. Bouchut.

The manifestations of the scrofulous diathesis are all exceedingly inveterate, and the treatment directed for their cure is only too often unsuccessful. All forms of scrofula, however, are not equally obstinate. The tertiary forms, which appear as tuberculosis, are generally incurable; of the other forms, whether primary or secondary, those which affect the bones are especially tenacious and of long duration; and, in truth, it is only over the mucous, glandular, and cutaneous manifestation of scrofula, that therapeutics can exert any decided power. The author has used in turn, all the medicines usually reputed anti-scrofulous: iodine, mercury, iron, baryta, bromine, extract of walnut leaves, cod oil, arsenic, &c.; but, of all these agents, the arseniate of soda has appeared to him to be, under the circumstances which he describes, the most energetic and the most efficacious. In suitable doses, arsenic is one of the best *tonics* and *corroborants* we possess; it is only in too large doses, or when its use is too long continued, that it deserves to figure as an *alterative*. As a tonic it is an admirable succedaneum of iron, quinine, and cod-liver oil; and is of great service in the majority of organic and nervous cachexiæ. In the scrofulous cachexia it is an excellent remedy; and under its influence children ordinarily regain their appetite, strength, and colour. In such cases there is amelioration only; but the cases where it cures are those in which the diathesis has not as yet produced a cachectic condition, and where the local manifestations are superficial and confined to the skin, the mucous membranes, and suppurated lymphatic glands. Beyond these, in tuberculosis and the diseases of bones, it is only a good palliative. But, although thus limited, the therapeutic effects have considerable importance. It is no slight

matter to be able to abridge the duration of a coryza, an ophthalmia, or the suppurations of glands, cutaneous ulcers, otorrhœas, leucorrhœas, &c., which depend upon the scrofulous constitution. The author possesses a number of facts relating to such cases, as well as to scrofulous perforation of the palate, reputed syphilitic, suppurating cervical glands (formerly called *écrouelles*), &c. The result in all of them was the same; a rapid cicatrization of the sores was always observed. The following are some examples:—

Obs. I.—A female child, *aged* 10, entered the Hospital Ste. Eugénie on 3d June, 1858; born of scrofulous parents; pale, thin, and weak, with an open scrofulous sore under the chin, discharging an abundant suppuration; below the angle of the jaw, other enlarged glands, not suppurated; appetite and digestion natural. Arseniate of soda was prescribed in increasing doses, from 5, 10, 15, to 20 milligrammes (1-14th to 1-4th grain) in a julep. Taken from July to December, the medicine produced no disturbance in the digestive organs. On October 15 the child was completely cured, and was dismissed on December 27. The tumours of the neck had disappeared, and there remained a solid cicatrix of good appearance under the chin.

Obs. II.—Girl of 13; two large scrofulous ulcers in the neck, with abundant suppuration, and the skin undermined. Arseniate of soda in increasing doses. Cure in three months.

Obs. III.—A girl of 10, suffering from osteitis of the phalanges of the big toe, onyxis, and suppuration of the matrix of the nail; treated with arseniate of soda internally, and foot-baths of chlorate of potash. In three months the wound had completely healed, although there still remained some swelling of the toe, and the skin was red and tender. The child had gained colour, had a keen appetite, and had become stout and plump.

The fourth case, a girl of 11, affected with lichen, and presenting numerous glandular enlargements and fistulous ulcers under the jaws, had a chronic abscess of the chest, which had been punctured and injected with iodine. She had been treated with cod-liver oil for three months, but had lost flesh, and the abscesses continued to discharge an abundant suppuration of an unhealthy character. On May 15, the arseniate of soda was commenced. On July 15, the abscesses were almost cured; and the child had a fresh healthy colour, and had gained flesh. On September 1, the abscesses on the neck and chest were completely closed. The cure continuing, the child was dismissed, Nov. 8. Since that date all the scrofulous children presenting superficial cutaneous affections in M. Bouchut's wards have been treated in the same way, and the result has always been favourable.

In the case of a girl, aged 11, affected with swelled submaxillary glands, and large scrofulous ulcers extending from the ear downwards under the chin on both sides, the improvement began after a fort-

night's treatment with the arseniate of soda, and she left the Hospital cured in four months.

Another girl, the subject of tinea tonsurans and osteitis of the big toe, had been ill for a year, and entered the Hospital, July 8, 1858. The general health was pretty good; with the exception of some paleness, there was no functional disorder. Epilation, lotions of corrosive sublimate, applied to the head; arseniate of soda internally; sulphurous baths. In November, 1859, the sore of the foot had completely cicatrized; but the tinea tonsurans had spread. The epilation was continued, and she was dismissed cured on the 15th October, 1860.

In all these cases the arseniate of soda was begun in doses of 5 milligrammes (1-14th grain), and increased at the end of a few days to 10, 15, and finally 20 milligrammes (¼ grain). Beyond that dose, symptoms of gastralgia, vomiting, and diarrhœa are apt to come on, and should be guarded against. It may be given in gum julep, in Bourdeaux wine, in syrup of cinchona, or syrup of gum. The following formula may be employed, and the medicine left in the charge of families, for use during several weeks:—Syrup of cinchona, 300 grammes (℥x); arseniate of soda, 5 centigrammes (gr. j.): one or two teaspoonfuls each day; each teaspoonful containing about 1 milligramme (1-70th grain) of the arseniate of soda. In this dose, and with the precaution of increasing it gradually, the arseniate of soda presents no danger. Its effects are, to increase the appetite and produce a richer sanguification, manifested by a ruddy colour of the skin, muscular energy, and an unmistakeable appearance of health. Such results, says the author, are not to be despised in the case of scrofulous subjects, pale, emaciated, and exhausted by long suppurations and mucous discharges; and it is on these grounds that he recommends the arseniate. He does not propose it, however, as a specific, but only as a tonic or corroborant, which stimulates the appetite and imparts increased activity to the molecular nutrition of the tissues. In scrofulous constitutions, it is the slowness of the movements of nutrition, and of the exchange of the circulating materials, which gives the diseases their peculiarly chronic character. In this respect the arsenical medication is useful, as perhaps the cod-liver oil is also, by stimulating nutrition; and the results obtained should induce practitioners to have recourse to it. It must be noted, however, that the arseniate of soda is suitable only in scrofulous diseases of the cutaneous, mucous, and glandular textures. Its efficacy is doubtful in diseases of the bones; and it is only a palliative in the case of tertiary scrofula, that is, in tuberculization.—*Bull. Gen. de Therapeutique.—Edinburgh Medical Journal, March*, 1861, p. 830.

5.—ON THE CONNEXION BETWEEN GOUT AND DEGENERATION OF ARTERIES.

By Edward Canton, Esq., Surgeon to the Charing Cross Hospital.

[A great many authors regard gout as a fertile source of atrophy and degeneration of arteries. An opinion of some weight is that of Dr. Hope, who states gout to be "an affection remarkably often attended with arterial ossifications, and their frequent concomitant, angina pectoris. Mr. Canton, however, considers that we may more rationally look to the subversion of normal nutrition of texture, which springs from malassimilation in the digestive process.]

Amongst the diseases which give rise to atrophy and degeneration of the arteries, gout is specified by Otto; and this malady is believed by Andral to be a very prolific source of these changes in the blood-vessels. "Gout," observes Dr. Copland, "generally originates in an excessive use of animal food, conjoined with deficient assimilative and secreting powers of the frame. The highly azotized blood of a person thus circumstanced becomes surcharged with urea and phosphate of lime, as evinced by the state of the urinary secretion, which always, in such cases, abounds in uric acid and the earthy salts. The experiments of Magendie have proved that by changing the diet of a person who has been living chiefly on animal food, and by substituting substances containing no azote, the uric acid and phosphates disappear from the urine. May we not, therefore, infer that, in consequence of the excessive use of animal food, conjoined with imperfect assimilating and secreting powers, these substances will accumulate in the blood to a hurtful extent, the urinary organs being unable to eliminate them entirely from the circulating fluid? The necessary result of this state of the blood will be, that these substances will occasionally be deposited in other parts, giving origin to the uric acid concretions found in the small joints, and to the phosphate of lime found in the arterial system and some other parts."

While considering the subject of atrophy and degeneration of bloodvessels in connexion with gout, I may devote a short space to a subject which, at the present time, is prominently under the notice of the profession—viz, *glaucoma*, a disease, which, as far as my experience has extended, is closely allied to that particular form of inflammatory action known as *arthritic*. Nor is this opinion, by any means, a peculiar one; for the most eminent ophthalmologists, concurrently, agree in their expression of the same belief—e.g., Lawrence says: "Glaucoma certainly occurs more frequently in such persons as are liable to gout than others." The cases related by Sichel are chiefly those of glaucoma, with arthritic inflammation. Mackenzie remarks, that "the subjects of glaucoma often labour under the symptoms of irregular gout, and, not unfrequently, have long suffered from those pains in the teeth and head which are generally accounted rheumatic. The Germans appear to consider glaucoma as almost

always connected with arthritis. or rather as the result of slow arthritic inflammation of the eye, especially the choroid." Tyrrell believes the disease to take place during suppressed gout. and speaks of the alternation of the two diseases. He relates cases also, showing clearly the association of gout with glaucoma. I have frequently heard the late Mr. Guthrie express an opinion that glaucoma is to be regarded as a gouty affection. "Glaucoma," observes Beer, "and green cataract, considered as consequences of inflammation, belong to arthritic inflammation of the globe; and the same form of disease, when occurring without inflammation, are only seen in the gouty."

Mr. Hulke states, on the other hand, that he has been unable to trace any connexion between glaucoma and gout and rheumatism.

Of the glaucomatous eyes which were dissected by Elbe, one was from a woman who had suffered much from gout, and the other from a man who had frequently experienced arthritic inflammation of the eyes. Of the specimens examined by Rosas, Lawrence remarks—"they must have been cases of arthritic inflammation of the internal tunics, and chiefly in their chronic stage."

Although I believe the true arthritic nature of an attack of acute glaucoma is not, at present. determined with such precision as we might desire it to be, in order to enable us to speak of the disease as an undoubted example of gout invading the eye, the symptoms nevertheless are very closely allied to those which commonly characterize this latter complaint when elsewhere occurring: the sudden accession of pain of a racking character; the remissions. succeeded by fresh paroxysms; the swelling of superposed parts (chemosis of conjunctiva or tumefaction of the softer tissues of the toe); greatly increased vascularity (of synovial membrane or retina); augmented pulsation of vessels passing to the inflamed part (tibials, or centralis retinæ); effusion of fluid into the cavity of the eyeball or articulation; blood extravasated from over-engorgement of vessels in either case (this may be sometimes seen to have occurred, also, into the sub-conjunctival areolar tissue where arthritic inflammation attacks this membrane).

In the dissections made of eyes affected by glaucoma, has inquiry been instituted respecting the presence of urate of soda in any of the humours or tunics?

The impairment of the nutrition of the bloodvessels of the retina is well illustrated by the fact—first noticed, here, by Mr. Bowman—of the pouched or aneurismal condition they assume in glaucoma; yielding, and even at last bursting, from diminished power to sustain their contents. "The first observations of this state," remarks Mr. Paget, "were published, I believe, by Kölliker and Hasse. in an account of a case of inflammatory red softening of the brain in which many of what, at first sight, appeared to be points of extravasated blood, proved to be dilatations of capillary vessels filled with blood. After this, they found the same changes, but in a much less degree, in some cases of inflammation artificially excited in the brain of rabbits and pigeons."

In acute glaucoma neither atheroma nor calcification of the retinal vessels exists; but Mr. Hulke observes—"The subsequent changes in the choroid and retina, in the advanced stages of the disease, have an atrophied character. In some cases I minutely examined in the summer of 1856, I found the dilated capillaries of the retina had become dark and granular; they were charged with minute oil globules, and often contained a few grains and little masses of red pigment. In short, these vessels were in a state of fatty degeneration, and the neighbouring parts supplied by them participated in the atrophy, and formed small spots of dull stone-grey colour."—*Lancet*, *Nov.* 17, 1860, *p.* 480.

DISEASES OF THE NERVOUS SYSTEM.

6.—ON SOME OF THE MORE UNUSUAL FORMS OF PARALYSIS.

By Dr. Wm. Roberts, Physician to the Manchester Royal Infirmary.

[The most frequent and serious cases of paralysis are those arising from some interruption of the line of communication between the volitional centres and the muscles, from inflammation and its consequences, from pressure by tumours, depressed fractures, effusions, &c. There are other cases about which, until of late years very little has been known, in which no interruption can be found, (except as a result and not a determining cause). The following groups will be found to embrace nearly all these cases:]

Group 1. Paralysis from mineral poisons, lead, arsenic, copper, &c.
„ 2. Wasting palsy (*Atrophie musculaire progressive* of Aran).
„ 3. Reflex paralysis—including
Infantile paralysis (*Paralysie Essentielle* of Rilliet).
Paralysis following the endemic colic of hot countries.
Paralysis following zymotic diseases, cholera, dysentery, continued and eruptive fevers, diphtheria, &c.
Facial paralysis, and certain other local palsies.
Hysterical and emotional paralysis.
„ 4. Paralysis affecting the co-ordinating power, (*Ataxie Musculaire locomotrice* of Duchenne).

It is not my purpose, in the ensuing pages, to enter on a full consideration of each of these groups, but merely to offer some contributions to the study of the less known among them in the form of comments on cases that have fallen under my own observation.

The first group of cases to which the reader's attention is desired are examples of Wasting Palsy. The necessity of erecting this class of cases into a distinct nosological species was first pointed out by Cruveilhier, in 1848, and forcibly advocated in the able Essay of Aran, in 1853.

The characteristic of wasting palsy is a gradual loss of motive power from atrophic degeneration of the muscles, independent of any disease of the brain or cord. The volitional impulses proceed to the muscles without impediment, but the decaying fibres are no longer able to contract in response. The wasting may extend to nearly all the voluntary muscles, both of the trunk and extremities, or be confined to one or more groups in the upper or lower limbs.

This led Aran to divide the cases into two divisions, according as the atrophy was partial or general. Practically it is of importance to keep the two groups distinct. The gravity of wasting palsy, *so far as the part is concerned*, is commensurate with its extent, *but so far as life is concerned*, it depends on the location. So long as the disease is limited to the extremities, life is not imperilled, but so soon as the muscles of respiration are attacked, the prognosis becomes exceedingly grave, for death is the usual result.

General wasting palsy differs also from the partial variety, not merely in the extent and locality of its ravages, but in its course and condition of origin; so that there is good reason, apart from the contrasted prognosis, to consider the two groups as distinct varieties.

Since the publication of my essay, a considerable number of cases of wasting palsy have fallen under my notice; but all except two presented the disease in a partial form. Some of the most interesting of these I shall briefly relate: accompanying the recital with a running comment on the most important points.

Afterwards I propose to bring forward some illustrations of reflex paralysis, especially as it occurs in infancy and childhood, and finally some cases exhibiting loss of the co-ordinating power over the voluntary muscles.

[*Case* 1, is a little boy aged 8, who, five years ago, had two fits, at intervals of six months. He continued well, however, till a year since, when his arm was noticed hanging almost powerless at his side. After this the muscle gradually wasted.]

About six months from the debut of the complaint the patient entered the infirmary for the first time. At that period his condition answered to the following description:—He was a well grown lad, standing upright, and walking without the least impediment. The chest and right arm were perfectly developed. The left arm, however, was the seat of a very serious loss of power and *great wasting*. The scapulo-humeral muscles, especially the deltoid, were affected to that degree that there remained scarcely any power to lift the arm from the side. The muscles moving the elbow joint were also deeply injured, but the unweighted forearm could still be flexed and extended completely although slowly and feebly. The forearm and hand were comparatively free, though considerably inferior in bulk and power to the corresponding parts of the opposite limb. The left pectorals were

somewhat touched. The general health was not quite satisfactory. There was a langour of mind and body, not usual to the boy; the tongue presented a dirty yellow fur, and the appetite failed somewhat from time to time.

He was diligently galvanized through the affected muscles every other day. A stimulating liniment was also rubbed in on the alternate days, and the citrate of iron and quinine was administered internally.

He resided in the infirmary, undergoing this treatment, for a period of three months. No advantage was gained; on the contrary the disease made slow but alarming progress. Before his discharge it was distinctly ascertained that the right shoulder was becoming affected; and a slight inclination of the head to the right indicated but too surely that the muscles on the left side of the neck were becoming too weak to withstand the tension of their antagonists. In this state he was sent home at the request of his parents. After an absence of seven months his father brought the patient again; and he was admitted a second time into the infirmary, on February 13, 1860. During this interval the disease had fulfilled the gloomy prognostications entertained at his discharge. The gait had now become decidedly halting owing to a want of power in the left lower limb; the head had fallen over to the right side and rested on the right shoulder, and scarcely any power remained to lift it into the erect position. The spine was twice sharply curved in a reversed direction in the cervical and lumbar regions, in such fashion as to preserve the general equilibrium. The unsightly deformity thus produced disappeared entirely when the patient was held up by the head or stretched out on the bed. The condition of the muscular system is thus described in the notes of the case:—

"The muscles of both upper extremities are wasted extremely from the shoulders to the hand. On the left side the scapular muscles can hardly be said to exist. The deltoid gives no response to the strongest galvanic shocks; the pectorals answer still, though feebly. The little power that remains in the larger pectoral overpowers the scapulo-humeral muscles and serves to draw the upper part of the humerus upwards and forwards; so that owing to the distinctness with which the prominences of bone about the shoulder can be seen, from the atrophy of the deltoid, the head of the bone looks as if displaced upwards and forwards between the acromion and coracoid processes. The anterior and posterior boundaries of the axilla are represented by thin webs of integument. The biceps contracts with scarcely sufficient force to flex the arm, and its antagonist is but just equal to the task of extending it under the influence of galvanism. The brach: Ant: and the flexors and extensors of the wrist and fingers are much in the same state. The thenar and hypothenar eminences are gone and the grip is altogether lost.

"The right upper limb is in a very similar condition, but the disease has not advanced so far; especially in the forearm and hand, which still possess some share of power.

"The head has dropped from atrophy of the left splenius and trapezius and rests on the right shoulder; it has also fallen a little forward.

"The rhomboidei and serratus magnus on both sides are implicated; and the muscular mass in the vertebral groove shows signs fo weakened power, particularly on the right side.

"It is easy to understand that with this failure of muscular force the lad has great difficulty, when lying, on his back, in raising himself to a sitting posture. He can only do so in fact by first turning himself to the right and then calling forth all the power still left in the right arm; by this manœuvre he just manages to accomplish his object. When going about the ward he pulls the left leg after him with a bad limping motion. The muscular power of the whole left lower limb seems weakened, but there is scarcely an appreciable atrophy of it; the difference between the circumference of the two thighs being not more than 3-8ths of an inch in favour of the right. He frequently tumbles in walking, and when this happens he appears bewildered, or as one of the patients in the ward expressed it 'knocked senseless,' and he lies on his face until some one picks him up.

"The destruction of the muscles of the hands and arms has reduced him to complete helplessness. He can neither dress nor undress alone. Not a morsel or drop can he convey to his mouth—the attendants having to feed him at every meal. The organic functions are altogether undisturbed. The muscles of the face, with those of the eye, the tongue, and the pharynx have entirely escaped. Pain is never complained of. No muscular vibrations have ever been witnessed, although the malady has been watched while in active progress for many consecutive months."

From the total failure of treatment in the first instance, but little expectation was held out that the disease could be cured; but it was hoped that its rapid progress might be stayed, and a state of permanent arrest be brought about, which would at least leave life intact. Cod liver oil, with the syrup of the iodide of iron, was administered internally; and he was subjected four times a week, either by myself or one of the pupils, properly instructed, to a thorough stimulation with the secondary galvanic current passed through the affected muscles. By patient perseverance, a very notable amendment was brought about. The disease made no progress at any point after his second admission; on the contrary, it has distinctly retrograded in nearly every region. In the course of the last six months, during which the patient has continued, with the exception of a month, an inmate of the infirmary, he has recovered the power and volume of the muscles of the right arm and hand to a very great extent; so that he can feed himself with ease, and there is a very considerable

force of grasp. The left is still very feeble, but he can readily lift the hand to the mouth—a movement which he could not attempt when at the worst. The head can be lifted into a nearly upright position by voluntary effort, and I think it could be wholly so, but for the stiffening of the vertebral joints, and the contraction of the muscles on the right side of the neck, from the long-continued malposition of the head.

He raises himself from the recumbent position with facility ; he walks with confidence, and has lost that constant tendency to tumble, which at one time almost put a stop to his progression altogether. He still limps badly. His general health is now perfect, and the rosy colour is restored to his cheeks.

This case is exceptional in the suddenness of its invasion. Wasting palsy is remarkable for its slow, insidious onset ; but here we are told that 'one day' the arm was found powerless at the boy's side ; and it is positively asserted that there was no wasting of the limb at that time. When I saw the patient six months later there was very great wasting, and yet the father was not aware of it. It might, therefore, be questioned whether the muscles were not already affected before the discovery of the palsy. I am not, however, disposed to accept this explanation, but rather to attribute the original loss of power to an attack of infantile paralysis, and to regard the muscular atrophy, afterwards so extensively diffused, as a subsequent affection, related to, but not identical with the original disease.

It will be remembered that the boy had already had, on two occasions, when 3 years of age, a fit accompanied with unconsciousness. The occurrence of these would seem to show a susceptibility of the nervous centres, such as we should expect to find associated with the production of infantile paralysis. When the wasting palsy, however, had fairly started on its own account, it followed the *progressive* course usual with that complaint. The deformity occasioned by atrophy of the muscles on the left side of the neck is very singular, and when the boy was admitted the second time, it was attributed to some organic disease of the cervical vertebræ, producing curvature. Error from this source was at once detected when the patient lay in bed : for then the spinal curvature could be readily undone, and the column restored to its proper shape.

A most gratifying, and unfortunately, far from usual, circumstance in the history of this case is the success of the treatment; and that under a most threatening aspect of the disease. On his second admission the wasting was rapidly extending; it had already deeply affected the neck and the right upper and the left lower limb, and there was every fear that the muscles of deglutition and respiration would not long escape. The daily application of galvanism, however, speedily arrested the onward course, and in about six weeks a perceptible amendment could be observed. That the amelioration was not due to any spontaneous curative process, but to the means employed, was

satisfactorily proved by a temporary intermission of the galvanism. The boy went out for a month, during the white-washing of the wards. On his return he appeared to be exactly as he was at his departure. On the resumption of the galvanism amendment again went forward as before.

It may not be without use to point out the precise method in which the galvanic current may be employed with the most advantage in cases of wasting palsy. The important thing is to pass the current through the substance of the affected muscles, and not along the nerves leading to them, nor into the spinal cord. This disease is regarded as a lesion of the nutritive operations in the muscle, and to the muscle itself, therefore, our remedy must be applied. To effect this the two poles of the battery should consist of small brass balls, closely covered with sponges. The sponges, thoroughly wetted, should should then be placed over the muscle to be galvanized, in close proximity to each other; that is, at a distance of an inch, or an inch-and-a-half apart, so that the current in passing from pole to pole may pass through the muscular substance. If the brass balls are applied dry to the surface of the skin, there is little penetration into the muscle—the current passing almost entirely along the integument; but when the wet sponges are pressed on the surface, the current passes deeply through the parts beneath. Again, every muscle ought to be galvanized more or less strongly as it has suffered in its nutrition. The more a muscle is atrophied, and its contractility diminished, the more strongly it should be galvanized; but when the contractility begins to return, it is better to moderate the strength of the current. The sittings should not be too prolonged, lest the muscles be exhausted, and evil produced instead of good. No sitting should be protracted beyond ten or fifteen minutes, and the affected muscles should be successively stimulated one after the other, the operator passing rapidly from muscle to muscle, and then returning to each until it has received its appropriate quota of stimulation.

The case we have been now considering belongs to that very serious class, happily very rare, in which the disease threatens to invade all the voluntary muscles, thereby endangering life, as well as destroyfng the usefulness of the limbs. But very far more common are the cases where the wasting is confined to a single group, or a limited number of groups of muscles in the upper extremities, or more rarely in the lower extremities, and where there is no tendency to a generalisation of the atrophy. Some of these present very odd deformities from the disappearance of the muscles in the afflicted region, while the remainder of the limb has preserved its volume. Sometimes the shoulders and upper arms of a patient are emaciated to the last degree, while the forearms and hands are plump and muscular, There is, at the present time, an example of this variety in the Royal Infirmary, under the care of my colleague, Dr. Browne.—*London Med. Review, Nov.* 1860, *p.* 212.

7.—ON THE DIAGNOSIS AND TREATMENT OF THE PRINCIPAL FORMS OF PARALYSIS OF THE LOWER EXTREMITIES.

By Dr. C. E. BROWN-SEQUARD, F.R.S.

[There are two distinct groups of cases of paraplegia, one distinguished by symptoms of irritation, the other characterised by the absence of these. The symptoms of irritation observed in the former class, are convulsions, cramp, twitchings, erection of the penis, formication, and itching. Diminution of temperature, wasting of muscles, œdema, bed-sores, and alkaline urine. In the second class all these symptoms are wanting, and the paraplegia is caused by the white or non-inflammatory softening, or is of the reflex kind. These two groups are, then,]

1st, Cases in which there are symptoms of irritation, with an increased amount of blood in the spinal cord or its membranes; 2nd, cases in which there is an insufficiency of nutrition, and most frequently an insufficiency in the amount of blood in the spinal cord, and also no symptoms of irritation. This division leads to the establishment of the following rules of treatment:—

1st. In those cases of paraplegia in which there are symptoms of irritation, employ those means of treatment that diminish the amount of blood in the vertebral canal.

2nd. In those cases of paraplegia in which there are no symptoms of irritation, employ those means of treatment which increase nutrition and the amount of blood in the spinal cord.

We are now led to examine by what means these two effects may be obtained—i. e., to increase or diminish the amount of blood or the nutrition of the spinal cord. With these rules in view, we will say a few words on each of the various means of treatment that are known to have been employed successfully in cases of paraplegia.

1. *Belladonna.*—This most powerful remedy has been employed quite blindly in the various forms of paraplegia by French and Italian physicians. The *rationale* of its mode of action is generally so little known, that it is often prescribed in those cases in which, instead of being useful, it increases the paralysis. An eminent author of a very learned work on Therapeutics and Pharmacology declares that "It is quite obvious that it (belladonna) should never be employed in cases dependent on congestion, inflammation, or organic lesion of the nervous centres, until this condition shall have ceased entirely, and nothing left but inertness." The truth is, that it is precisely in cases of congestion or inflammation of the spinal cord or of its membranes that belladonna should be used against paralysis. The mistake made by this most able writer depends in a measure upon the general but erroneous opinion that belladonna is a stimulant of the nervous centres. We will not speak here of its action on the brain; but as regards the spinal cord and the spinal nerves, belladonna, far from being a stimulant, acts in diminishing the vital properties of these organs. As we

have already said, belladonna is a powerful excitant of bloodvessels, and especially of those of the spinal cord and its membranes. In consequence of this influence, it diminishes the amount of blood in the vertebral canal, and in so doing produces a relative diminution of the vital properties of the spinal cord and its nerves. It is, therefore, in those cases in which these vital properties are increased that belladonna should be employed. We will sum up the indications to use or to avoid belladonna in saying:—

1st. Belladonna is one of the most powerful and reliable remedies that we may employ, in cases of paraplegia with symptoms of irritation of the motor, sensitive, and vaso-motor or nutritive nerve-fibres of the spinal cord, or of the roots of its nerves; in other words, in cases of congestion, meningitis, or myelitis.

2nd. Belladonna is a most dangerous agent, able only to increase the paralysis, if employed in cases of paraplegia, without symptoms of irritation, such as cases of white softening, or of the reflex paraplegia.

2. *Ergot of Rye*—This powerful remedy has also been employed quite blindly, although its mode of action being better known than that of belladonna, it would have been easier to discern in what cases of paraplegia it is able to be of benefit. Ergot, like belladonna, produces a contraction in the bloodvessels of the spinal cord and its membranes, and, therefore, diminishes the amount of blood circulating in these organs. The following is a summary of the indications and contra-indications to its use:—

1st. Ergot must be employed in cases of paraplegia with irritation of motor, sensitive, or vaso-motor nerves—i. e., in congestion or inflammation of the spinal cord or its meninges.

2nd. Ergot must be avoided as an agent only able to increase the paralysis in cases of paraplegia without symptoms of irritation, such as cases of the reflex paraplegia, or of non-inflammatory softening of the spinal cord.

3. *Strychnine.*—This most powerful remedy is also employed indiscriminately. I know of a large number of patients whose paralysis has been increased by the influence of this medicine. Its mode of action consists in increasing the amount of blood in the spinal cord and its membranes, and, as a consequence, in increasing the vital properties of that nervous centre. Therefore—

1st. Strychnine ought to be employed in those cases of paraplegia in which there is no sign of irritation, or of increase of the vital properties of the spinal cord, such as the cases of reflex paraplegia and of white softening of the spinal cord.

2nd. Strychnine ought to be avoided as a most dangerous poison in those cases of paraplegia in which there are signs of congestion or inflammation of the spinal cord or its meninges. In those cases strychnine can only increase the cause of the paralysis.

4. *Sulphur.*—The *modus agendi* of this medicine in paraplegia is not well known, but it is at any rate certain that sulphur may be em-

ployed with advantage in cases of reflex paraplegia or white softening of the spinal cord, in which there is no irritation of this nervous centre. My own experience confirms the assertions of Graves in that respect.

5. *Phosphorus.*—The mode of action of phosphorus is still less known than that of sulphur. German physicians have successfully made use of this agent in paraplegia, probably in cases of insufficiency in the amount of blood in the spinal cord. We must say, however, that we have tried it without any benefit in two cases of white softening of the spinal cord.

6. *Mercury.*—No agent has been more extensively and more blindly used than mercury in cases of paraplegia. It is only in cases with an increased amount of blood in the spinal cord or its meninges that mercury may be used with advantage. Its depressing influence, however, is such that I never employ it except in cases of syphilitic paraplegia. It is decidedly injurious in cases of white softening of the spinal cord, and also in cases of reflex paraplegia.

7. *Iodide of potassium.*—This remedy is not sufficiently employed in paralysis. It is one of the most powerful agents of absorption of fluids effused in the cranio-vertebral cavity, either out of or in the substance of the nervous centres. It is the only known remedy that may be employed without danger in the various forms of paraplegia. It is especially useful in cases of white softening of the spinal cord, due to the fatty degeneration of the bloodvessels of that organ. It has, more than mercury, the power of producing the absorption of effused fluids in the vertebral canal, and it is decidedly less depressing than mercury. In cases of syphilitic paraplegia its curative influence is sometimes very rapid.

8. *Cantharides.*—This is another remedy, the mode of action of which is ignored, and which is employed blindly in any form of paraplegia. I have tried it without any apparent benefit in cases of paraplegia belonging to the two most distinct forms of this affection—i.e., a case of white softening of the spinal cord, and a case of chronic meningitis. It seems, however, to have been useful in some cases of chronic myelitis.

9. *Stramonium, Hyoscyamus, and Indian Hemp.*—These agents act more or less like belladonna and ergot of rye on the bloodvessels of the spinal cord, and they are, therefore, to be used in those cases of paraplegia in which the amount of blood is increased in that organ. Of course they must be avoided in cases of white softening or of reflex paraplegia. Against insomnia in cases of paraplegia with symptoms of irritation of the spinal cord, hyoscyamus or Indian hemp ought to be employed, instead of opium, the use of which is then dangerous, as it produces a congestion of the spinal cord.

10. *Ammonia, Sulphate of Quinine, and Iron.*—These medicinal agents are sometimes of great service in paraplegia. It is chiefly in cases of insufficiency in the amount of blood and in the nutrition of the spinal cord that they are to be employed; but they may be also

used with benefit in cases of chronic myelitis or meningitis, or of spinal congestion, when the symptoms of irritation are not violent, if the pulse is weak and slow.

11. *Cod-liver Oil.*—Some cases of paraplegia have been reported in which cod-liver oil seems to have produced a very great amelioration. We have never employed it alone; but a notable amelioration has taken place in some cases of chronic myelitis in which we have employed cod-liver oil with belladonna and ergot of rye. In two of these cases, the patients had been treated for five or six weeks without amelioration by belladonna and ergot, and they began to get better only when cod-liver oil was added to the treatment.

12. *Bleeding and Cupping.*—The chronic inflammation of the spinal cord or its meninges does not require bleeding. Except in one case, we have never seen any decided good effect produced by bleeding in cases of paraplegia owing to chronic inflammation or to congestion. We hardly need say that any loss of blood might prove most injurious in cases of paraplegia in which there is a diminution in the amount of blood in the spinal cord or of the nutrition of that organ, as in cases of white softening or of reflex paraplegia. Dry cupping frequently applied to the spine is an excellent means of treatment in those cases in which there is either an inflammation or a congestion in the spinal cord or its membranes. It would rather be injurious than useful in cases of white softening or of reflex paraplegia.

13. *Blister, Moxa, Issue, Actual Cautery, &c.*—Revulsion, by these various means, is, to say the least, useless in cases of white softening of the spinal cord and of the reflex pareplegia. But these various means are often of great service in cases of myelitis or meningitis, especially when the inflammation is limited to a small extent in the length of the cord or its membranes.

14. *Cold and Hot Douche.*—The hot douche (at about 100°), applied *loco dolente* on the spine, is very useful in cases of inflammation or congestion, and useless, if not hurtful, in cases of reflex paraplegia and of white softening. As regards the cold douche, the cold shower bath, sponging with cold water, &c., they are certainly most beneficial in cases of white softening, or in the reflex paraplegia.

From the above review of the various means of treatment of paraplegia, it results:—

1st. That in cases of paralysis of the lower limbs, with symptoms of irritation of the motor, sensitive, and vaso-motor nerve-fibres of the spinal cord or of the roots of its nerves, the proper treatment consists in the use of some of the following agents or means:—Belladonna, ergot of rye, hyoscyamus, stramonium, Indian hemp, dry cupping, blisters, moxæ, issues, the hot douche, and also sometimes the iodide of potassium, ammonia, sulphate of quinine, iron, or cod-liver oil.

2nd. That in case of paraplegia without symptoms of irritation of the spinal cord or the roots of its nerves, the rational treatment consists in the use of strychnine, sulphur, the cold douche or shower-

bath, and also of the iodide of potassium, and frequently ammonia, quinine, and iron.

As regards the principal, I might say the essential, agents to be employed against paraplegia, I can now repeat what I have so often said in these lectures, that it is of the utmost importance in the treatment of this affection not to make use of strychnine, belladonna, mercury, &c., before having ascertained positively whether there are or not symptoms of irritation of the spinal cord. Giving strychnine indiscriminately in all cases of paraplegia, as is done by so many practitioners, is a very dangerous mode of proceeding, as it may increase notably the disease to which the paralysis is due. The same thing may be said of the indiscriminate use of mercury, belladonna, cantharides, ergot, &c.

We will not say more here as regards the treatment of the various forms of paraplegia, as we have already discussed at length, in the beginning of this lecture and in the two preceding, the means of improving the nutrition of the spinal cord, of preventing the alterations of nutrition in the paralysed limbs, in the bladder, &c., and also of diminishing the external irritation in cases of reflex paraplegia.

General Conclusions of the Course.—Our principal object in these lectures has been to show—

1st. That there is a form of paralysis of the lower limbs entirely distinct from all others, as proved by its mode of production, by morbid anatomy, by its symptoms, and by the influence of a certain mode of treatment, and that this form of paralysis fully deserves the name of reflex pareplegia.

2nd. That the reflex pareplegia may be caused by the most various irritations of the skin, the mucous and serous membranes, the abdominal or thoracic viscera, as well as of the genital organs or the trunks of the spinal nerves.

3rd. That most cases of paraplegia can be placed in two groups, entirely different one from the other, according to the existence or the absence of symptoms of irritation of the motor, sensitive, and vaso-motor nerve-fibres.

4th. That most of the therapeutical means to be employed in paraplegia are also to be grouped in two categories, one of which is fitted to those cases in which there are symptoms of irritation, and the other to those cases in which these symptoms do not exist.—*Lancet, Dec.* 22, 1860, *p.* 605.

8.—ON THE DIAGNOSTIC VALUE OF THE SYMPTOMS OF DISEASES OF THE BRAIN.

By Dr. Brown-Sequard.

[The following is an abstract of one of the Gulstonian lectures for this year. Speaking of the diseases of the base of the encephalon, the author observes :]

A very important observation has been made—that all the nerves entering the brain through the base of the skull decussate near their entrance into the encephalon; and after the decussation, most of them send fibres upwards into the brain. Knowing this, we may first determine theoretically what symptoms should be produced by injury of a given part; and then examine how far this theory of symptoms agrees with what is actually observed.

With the exception of the auditory nerve, the cranial nerves springing from the *medulla oblongata* have their origin at the back rather than at the front of this part of the encephalon. All these nerves pass into the grey matter, and there undergo partial decussation. To illustrate the effects of disease on these, according to its seat, the lecturer selected the sixth nerve.

Diseases affecting the medulla just at the origin of the sixth nerve on one side will produce paralysis of the external rectus on the same side as the injury; while there will be paralysis of the opposite side of the body. This agrees with what is observed in practice. But if it be inflammation that affects the origin of the sixth nerve, there will be convulsive action of the muscle supplied by it, and the eye will be drawn outwards. Again, if this nerve be injured after its decussation in the pons Varolii, the external rectus of the other side is paralysed; and the paralysis of the muscle of the eye here occurs on the same side as the general paralysis of the body and limbs. In another class of cases, there may be an injury on one side, small in extent, yet near enough to the point of decussation to affect the nerves on both sides. Here there will be paralysis of both external recti, although one side of the body is paralysed.

Analogous phenomena are observed when other of the cranial nerves are affected. The phenomena of "alternate paralysis" (paralysis affecting partly one side of the body and partly the other) have lately been made the subject of a paper by M. Gubler. M. Brown-Séquard, however, had given an explanation of the fact ten years ago; and even then he did not think he was advancing any new doctrine. The phenomena had also been explained by Romberg and others prior to the appearance of M. Gubler's essay. M. Gubler has collected cases of injury at the lower part of the *pons Varolii*, affecting the facial and trigeminal nerves, and producing paralysis of the parts supplied by these nerves on the same side; while the opposite side of the body has been paralysed. It would appear, however, that M. Gubler has drawn too general a conclusion from these facts; for, if the injury be at the level of the decussation of the nerves, then the paralysis of the parts supplied by them occurs on both sides. Two cases of this kind have been recorded. Again, injury of the pons above the point of decussation of these nerves will produce paralysis of the face on the opposite side—on the same side as that on which the general paralysis of the body occurs.

The effect of injury of the pons Varolii in producing paralysis of

the cranial nerves then varies according to its situation. When the injury is below the point of decussation, the paralysis is on the same side; when it is above, the paralysis is on the opposite side; and when it is between these, so as to affect the nerves just where they decussate, the paralysis of the parts supplied by the nerves involved occurs on both sides.

Are, then, these signs sufficient for the positive diagnosis of paralysis originating in injury of the pons Varolii? No; but there are other symptoms which aid in the diagnosis.

If the injury of the pons be produced rapidly, as by effusion of blood, the breathing will soon be affected. Again, in the paralysed parts there is a remarkable diminution of temperature; and, as a general rule, the presence of an irritation in the pons and perhaps in some part of the medulla oblongata, is denoted by a considerable loss of heat on one side of the body. If, however, there be no irritation, the temperature in the paralysed limbs will be increased. Again, if there be paralysis of the muscles of the eye, attended with constriction of the pupil, there is an irritation somewhere; and, if there be no other obvious source for this, it must be located in the pons. Further, in most cases of simple injury of the pons, the intelligence remains normal: and there is difficulty in deglutition, and in the motion of the tongue. It is remarkable, however, that it is only when the lower part of the pons Varolii is injured, that the function of the hypoglossal nerve is much impaired.

The small size of the *anterior pyramids* in the medulla oblongata at first sight renders it difficult to believe that they are the main channel of the motor fibres in their passage from the brain to the spinal cord. Yet injuries of the other parts of the medulla do not paralyse; while an injury of the anterior pyramid of one side produces complete paralysis of motion of the opposite side of the body, Pathological facts also show that these anterior pyramids are the continuation of the lateral columns of the cord, and that a decussation takes place in them; for injury of the anterior columns of the cord just below the pyramids does not produce paralysis, while an injury of one lateral column produces paralysis of the body on the same side. The fact of a decussation of motor fibres in the anterior pyramids is also shown in the result of injury of the pons Varolii. If decussation of the nervous fibres did not take place before their entrance into this part, then injury of one side of the pons ought to produce paralysis of the same side of the body; whereas the contrary occurs, as has been shown in sixty or seventy cases.

The symptoms of irritation in the pons and medulla often manifest themselves in the form of convulsions. But it does not follow, because the convulsions may be epileptiform, that the actual seat of of epilepsy is here. The medulla is only the starting-point of an irritation; the seat of the epilepsy may vary. The first convulsions are observed to occur sometimes in one and sometimes in another

muscle; and occasionally the first result of the irritation is contraction of the blood-vessels of the brain, producing loss of consciousness.—*British Med. Journal, March* 2, 1861, *p.* 230.

9.—ON THE TREATMENT OF NEURALGIA.

By Dr. E. H. Sieveking, Physician to St. Mary's Hospital.

[Speaking of the hypodermic treatment of neuralgia, the writer observes that the dose thus given must be very carefully graduated, and its effects watched, the absorption from the areolar tissue being so much more ready than from the stomach.]

Within a few minutes after the solution has been thrown under the skin, the patient begins to feel the general effects of the narcotic; there is giddiness, followed by more or less drowsiness, and there may be the retching, sickness, and prostration after a time, which are occasionally the troublesome effects of the administration of opiates by the mouth. According to the idiosyncrasy of the indivinual, no less than according to the dose administered, these symptoms vary in intensity and duration; but they have appeared to me to occur more frequently than when similar doses are given by the mouth. The general symptoms are sometimes so severe as to prevent the patient from submitting to the injection again, even though there may have been great alleviation of the neuralgia. More frequently, however, this is not the case, and the relief afforded by the injection induces the sufferer even to apply for its repetition. With regard to its local effect, I can speak of it in terms of praise. Applied, as advised by Dr. Wood, at the point of greatest tenderness, which, for instance in sciatica, is generally at the sciatic notch, the anodyne does not fail to arrest the pain completely for a time, but it generally requires repetition before long. The irritation arising from frequent injections in the same locality may be avoided by varying the site of injection, and by using very concentrated solutions of morphia. If a solution of four grains to the drachm be used, seven minims and a half (equivalent to half a grain) would be the quantity suitable for an adult. The syringe should have a fine perforated nozzle, which supersedes the necessity of a canula and trocar, and permits of the injection being made with the smallest possible opening. I have not myself employed other solutions than those of morphia for hypodermic treatment, and do not understand how the method can be of very general use for the introduction of remedial agents into the system, as the majority would induce so great an amount of local irritation as to prevent their beneficial operation. This objection would apply strongly to such substances as quinine, which it was proposed in America to introduce in large doses subcutaneously. While I regard the hypodermic employment of morphia as a valuable addition to our therapeutic armamentarium, I do not think it ought to be indiscriminately used either for the purpose of

allaying pain or of procuring sleep. As a local anodyne, I consider the endermic application of morphia in the majority of instances preferable; but where this fails, or where the symptoms are so urgent as to require instant relief, there we may have recourse to the syringe.

A very few words may comprise what I have to say regarding chloroform in these cases. For the purposes of anæsthesia it may be administered in severe and enduring paroxysms in the presence of a medical man; the necessity for a frequent repetition, the risk attending its administration in large doses by the patient or his friends, and the brief respite from pain which it affords, prevent the inhalation of chloroform from being largely used in the treatment of neuralgia; as a temporary palliative of course it is excellent, and in the hands of an experienced practitioner it cannot fail to be of much use. Anodyne liniments containing chloroform are in many cases valuable adjuncts in the treatment of neuralgic affections.

Amongst the many topical applications that are made to the unbroken surface there are two upon which I would briefly dwell; the one is belladonna, the other veratria. Of the former I have a high opinion; I hold it to be of more use as a topical application to the unbroken skin than preparations of opium. Whether it is more readily absorbed, or whether it possesses specific powers, I know not; but the frequent, almost daily, service that liniments and ointments containing belladonna have done me justifies the encomium. Veratria too, though not generally employed, is a remedy which exercises a controlling influence over neuralgia; if an ointment containing from one to two grains to the ounce of lard be well rubbed into the seat of pain, the neuralgia is deadened, and generally a rash appears on the part. The following two cases serve as illustrations of the use of this agent:—

Neuralgia intercostalis.—W. C——, a waiter, aged twenty-five, applied for relief in September 4th, 1856, on account of pain in the lower left thorax, to which he had been subject off and on for two years. For the last three months it has been so bad as to force him to leave off work; for a fortnight he has had a cough, owing to a cold. During the last three months there have been frequent night sweats. There is a total loss of appetite, but no nausea or sickness. At times he suffers pain after eating, but not always. There is frequent palpitation, and pain on slight exertion. Four months ago he had a swelling of the right leg for three days, which has not returned. His face is large and bloated in the morning. The urine is dark, but there is no pain in the back. There is no trace of heart disease. The tongue is covered with a white fur; the pulse 92, feeble. The patient had a urethral discharge nine years ago, but never had primary chancre or secondary syphilis; though troubled with frequent nocturnal emissions, is not guilty of masturbation. The bowels have lately been relaxed. The pain is confined to the cartilages of the false ribs, which seem slightly enlarged on the left side. An ointment composed of four grains of veratria to two ounces of lard, to be rubbed into the

painful parts morning and night; and an ounce and a half of rhubarb and magnesia mixture to be taken every morning.

Sept. 16th. The ointment has been rubbed in three times a day with great relief to pain; it produced no rash or other visible effect on the surface. Tongue white; appetite poor. Ordered fifteen grains of citrate of iron to an ounce of the compound infusion of gentian three times a day; and to repeat the ointment.

19th. There was a slight return of pain last night. Twenty grains of the sesquioxide of iron with treacle were given thrice a day.

26th. Has had the pain three times for one or two minutes since last visit. His general health is much improved. The ointment has been rubbed in regularly; has caused the skin to be numbed, but has produced no rash. Repeat electuary and ointment.

Oct. 3rd. Has no pain. Repeat electuary.

17th. There is still some palpitation. Ordered infusion of digitalis and quinine mixture, of each half an ounce, three times a day.

Nov. 7th. Is much better; has had no pain for three weeks, but at times there is still palpitation. Apply belladonna plaster to the left thorax, and repeat electuary.

He continued under treatment until November 28th, at which time there had been no return of the pain. He was therefore discharged cured.

This is a genuine case of neuralgia, in which it was necessary in the first instance to eliminate the possibility of organic disease before determining upon the treatment to be adopted. Had a lowering system been pursued, the neuralgia would have been perpetuated, if not actually converted into an organic disorder. Had any doubt existed with regard to the diagnosis, the effect of the treatment must have dispelled it; while the benefit of the local application of veratria also stands out in relief, as it was employed in the first instance without any internal anti-neuralgic remedy. I subjoin one more case illustrative of the use of veratria.

Neuralgia faciei.—H. H., aged 39. the wife of a mason. She had had rheumatic fever four years before applying for aid, and has been in feeble health ever since. For six weeks past she has suffered from pain in the left side of the face. There are no carious teeth. The pain commences in the left lower incisors, and shoots to the neck and ear. It is worse by day, and is brought on by the least exertion. The patient is nursing a baby of three months old, and has a cough with expectoration, but there is no thoracic mischief. Pulse 80.

June 24th. Ordered, tincture of iron, fifteen minims; chloric ether, twenty minims; quinine mixture, one ounce: thrice a day; and five grains of compound colocynth pill every night.

30th. The face is much better. She has had no pain since the second day of taking the medicine, and there is a better supply of milk.

The patient continued the same treatment to July 18th, when she

is reported as having a return of the pain in the left side of the face, which although less than formerly, is still very severe, and causes the eyes to water. The veratria ointment to be rubbed into the seat of pain every night. Repeat the mixture.

July 29th. She states that the ointment "seemed to draw the pain out of the face;" that it caused a smarting pain for about half an hour, and a slight rash on the surface.

She continued, with slight variations, under treatment till Sept. 5th, when the report is that the veratria has brought out a rash on the face, and that she has had no severe attacks since employing it. She still has a slight pain flying about the face; and if she takes anything warm into her mouth she experiences a pain in the right side. Quinine mixture was now ordered, and the patient ceased from her attendance.

In this case we have to deal with a variety of debilitating causes, each sufficient to serve as a substratum for neuralgia. These had to be met by general tonics. That the local disorder was successfully combated by the local application as well, scarcely admits of a doubt, unless the patient's words are discredited.—*Lancet, Feb.* 9, 1861, *p.* 131.

10.—ON CHOREA.

By Dr. C. H. JONES, F.R.S., Physician to St. Mary's Hospital.

With regard to the essential nature of chorea, it seems to me that we shall not be far from the truth if we keep in mind the well established law, that usually mobility and debility of nervous tissue go hand in hand, that the weak structure is unduly sensitive and excitable. To determine the exact seat of the affection is not possible; and very probably it is not confined to any one locality. It is much more important to know the state of the nervous structures; and this, it seems pretty certain, is one of debility and exhaustion. This is the radical and necessary condition; but the causes which produce it may be very various. In the majority of cases, the nervous tissue falls of itself into a state of enfeeblement, or is reduced to it by ordinary influences, as bad air, poor food, depressing emotions, &c. In other cases, the impairment of nervous energy depends on worms or accumulations of fæces in the bowels, which, on the principle of inhibitory action or morbid stimulus, depress and enchain the nerve-force. In other cases, possibly some poison, by circulating in the blood, may conditionate the state of nerve-debility. The treatment must, of course, vary according to the presumed cause.

The connexion between chorea and rheumatism seems to be in some degree elucidated by viewing the latter as a disease in which the nervous system is materially involved. Without denying that acute rheumatism may depend on the presence of lactic acid in the blood, it yet seems clear that the ordinary inflammations of rheumatism mani-

fest peculiarities which can scarcely be accounted for except by the intimate concernment of the nervous system. Rheumatic affections shade, in fact, from the most decided severe, local, pus-generating inflammiations of pericardium or lungs, down to mere neuralgia with little or no vascular disorder. The more fugitive and shifting they are, the more of the nervous element one traces in them. This is not the place to discuss at length the arguments for this view; but if it be admitted that a state of paresis of the nervous system is common both to chorea and rheumatism—in one the cerebro-spinal department being affected, and in the other the vaso-motor—we have at once an important relation established between them. The inflammations of rheumatism might result from the mere paralysis of the articular vaso-motor nerves, without the presence of lactic acid in the blood; or, assuming that this is essential, its injurious effects might be greatly intensified by this nerve-disorder, or even developed in cases where otherwise they would have remained wanting.—*Brit. Med. Journal, Jan.* 5, 1861, *p.* 5.

11.—*On Chorea.*—[Chorea is a disease of extreme frequency at Birmingham. In thirty-five cases which occurred during four months at the Birmingham General Hospital, a rheumatic history was discoverable in all. A fact of great interest frequently observed, is the localisation of chorea and rheumatism in the same limb.]

It may be interesting to state the treatment of the General Hospital, and which still remains without much change. Decoction of aloes (compound) was first given, thrice daily, in a quantity sufficient to move the bowels two or three times a day. This, together with the altered diet and confinement to bed, soon produced an amendment, and as long as this continued to be progressive no change was made. Sometimes, but rarely, a cure was thus affected; more often, the intestinal irritation, beneficial at first, seemed afterwards to produce an exacerbation. As soon, then, as the patient ceased to mend, one or two grains of sulphate of zinc were given thrice daily, in a convenient vehicle, the aloes being used from time to time if the bowels were costive. The dose of zinc was increased as often as the case seemed to be at a standstill, and it was found that it was rarely useful in a quantity much less than that required to produce nausea. The quantity thus given was sometimes very remarkable, delicate children taking upwards of half a drachm in the day without any unpleasant effect. The diet was liberal, but not excessive. When the zinc proved ineffectual, Fowler's solution was substituted for it. Dr. Wade, when house-physician, added to this plan the use of Indian hemp, as a means simply of allaying the movements when these were excessive; and this practice still maintains its ground.—*Brit. Med. Journal, March* 2, 1861, *p.* 235.

12.—*On the Treatment of Tetanus.* By Editor of 'Lancet.'—Of the different indications in the treatment of tetanus, besides the removal of any source of irritation, and the support of the general strength, but especially of the heart, two are prominently dwelt upon by writers—namely, to lessen the susceptibility of the nervous centres to any irritating influence which may exist, and to diminish the irritation by means calculated to depress nervous excitement. To accomplish the first, Dr. Wood, of Philadelphia, thinks the weight of testimony is greatly in favour of opium, notwithstanding the contrary judgment of some distinguished authors. It has probably been employed, he asserts, as one of the remedies in the great majority of cases. It has failed like other remedial agents, but in many cases all prove ineffective. The liquid forms, from their more ready absorption, are to be preferred to the solid. The second indication is fulfilled by measures that have a sedative influence on the nervous system, and by those which act revulsively. Into these we do not propose to enter, but we may state that the tobacco enema (half a drachm to the half-pint of boiling water) is highly praised by Dr. Wood. It is to be repeated in an hour, and then every two or three hours, till its relaxing effects are produced. The nervous system is easily impressed by the influence of tobaacco, as contrasted with opium; and we think that by-and-by when the question has been thoroughly tested, it will be found that tobacco or its alkaloid, nicotine, is of greater value than many other remedies in tetanus. The revulsive plan consists in blisters or caustic applications to the whole length of the spine; or ice may be advantageously employed. The latter remedy is asserted to have been the means of cure in sixteen out of seventeen cases under the care of Dr. Carpenter, of Suffolk County, New York.—*Lancet Dec.* 8, 1860, *p.* 560.

13.—*Cases Illustrating the Treatment of Tetanus.* Under the care of the Surgeon of the Middlesex Hospital.—[In the first case the patient was kept from the very commencement under the influence of aconite. After some days, the cessation of the medicine on account of its producing sickness, was not followed by any aggravation of the tetanic symptoms. When the system was almost worn out by want of sleep, subcutaneous injection of morphia procured rest and tranquillised the patient. In another case the right hand was much lacerated, the wheel of an omnibus having passed over it. In two weeks]

The wounds were healing favourably, when, on the 22nd of April, tetanic symptoms set in commencing with the usual stiffness about the neck and jaw. She was immediately put upon the aconite treatment; three minims, and afterwards five minims (Fleming's tincture), were administered every three hours, the pulse being carefully watched. At the same time, the hypodermic injection of morphia was used, one-third to half a grain of the acetate being given once

or twice daily, according to circumstances. Subsequently, aconitine was administered in the same manner, a forty-fifth to a thirtieth of a grain being injected under the skin of the arm. The diet was liberal —beef-tea, brandy, and wine, as much as could be taken.

The disease never became very acute. She had trismus, was not able to seperate the teeth more than half an inch, well marked risus sardonicus, twitchings of the face, and spasmodic catching of the breath and of the arms; but no general spasms, opisthotonos, &c. She continued for about a fortnight in nearly the same condition, then all the symptoms gradually and slowly abated, and she was discharged from the hospital, quite well, on the 12th of June.—*Lancet, Dec.* 8, 1860, *p.* 562.

14.—*Treatment of Epilepsy.*—Dr. Brown-Sequard generally commences the treatment of epilepsy by belladonna. The usual dose of this remedy for an adult is one quarter of a grain twice a-day in pill or mixture. It is very rarely indeed seen to produce any of its specific effects, as dilatation of the pupil, in cases of epilepsy. One patient complained of dimness of vision, whose pupils were evidently dilated by the drug; but this was the exception, proving the rule, as the case was not one of epilepsy or any convulsive disorder.

In the cases in which there appear to be a tendency in the fits to appear at regular intervals, for instance once a fortnight, Dr. Brown-Sequard prescribes quinine in large doses, *e. g.*, five, ten, and even fifteen grains, to be given at intervals, shortly before the fit is expected. By this means the fit is frequently prevented, and the patient goes on to the next, or even to a longer period. In reference to these large doses of quinine, it is well known that some temporary deafness will often follow, and curiously enough, Dr. Brown-Sequard states that there is a kind of deafness which the administration of this remedy in large doses will cure.

Another therapeutical means in epilepsy is the ligature, in cases in which the aura epileptica, arising from one of the limbs, is present. Dr. Brown-Sequard has two patients, both girls, about the age of nine years, in the hospital, in whom the fits are frequently stopped in this way. The ligature is kept constantly on the arm; when the child feels the warning, the nurse of the ward tightens the bandage, and the fit is prevented. The success in these cases has been very great. It is of great consequence to have the ligature in readiness, so that it may be tightened at once. Grasping the limb tightly will do in the absence of proper means, but it is much better to keep a bandage or folded handkerchief tied on the arm ready to be tightened. Dr. Brown-Sequard has invented an apparatus to encircle the arm, and to tighten by a screw, in order that the pressure may be quickly applied.

Dr. Brown-Sequard frequently uses the actual cautery locally in a

variety of nervous affections. In epilepsy, patients frequently complain of either a pain or a sensation proceeding from some part of the body. A woman, aged 20, had had fits for thirteen years; they invariably commenced with pain in the left side, just below the mamma. Dr. Brown-Sequard applied the cautery to this part in two or three places. The relief was most marked. It had not prevented the fits altogether, but it had reduced their number very considerably. Instead of having them every other day, she had them only once a week. The cauterising iron is heated to a white heat, and is then applied suddenly to the part once or twice. It appears to cause but trifling pain, and the patients do not seem at all to dread its repetition.—*Med. Times and Gazette, Oct.* 27, 1860, *p.* 407.

DISEASES OF THE ORGANS OF CIRCULATION.

15.—ON PERICARDITIS.

By Dr. W. T. GAIRDNER, Physician to the Royal Infirmary of Edinburgh.

[Friction-murmur *per se* can only be taken as evidence that the pericardium is roughened, not that it is inflamed or actively diseased. There can be no doubt that the white-lymph patches and their allied fibrinous deposits in the pericardium, at some period of their progress, produce murmur.]

During upwards of six years of almost constant attendance in the Edinburgh Royal Infirmary as physician, I have of course had under my care a great many instances of pericardial affections arising in the course of rheumatism, and a certain number also concurring with other constitutional or local diseases. Purely idiopathic pericarditis I have rarely, if ever witnessed; and I am much inclined to believe that (as morbid anatomy teaches) it very rarely occurs as a severe or clinically important form of disease. Pericarditis from direct injury, or from purulent affection of the blood after injuries; pericarditis following erysipelas, or pleuro-pneumonia; chronic or subacute pericarditis in connection with unhealthy suppurations in various parts of the body; with epidemic fevers, especially small-pox and scarlatina; and, finally, pericarditis associated with Bright's disease,—have all fallen more or less frequently within my observation, and in many of these last-mentioned forms the issue has been fatal. But of *rheumatic pericarditis* and of *pericarditis without previous disease*, acute or chronic, *I have not had under my own personal care a single fatal case.*.

Let me endeavour to indicate, shortly, what has been my usual course of proceeding in dealing with such cases. In every such case of rheumatic disease, whether acute or chronic, I have made, as a rule, at the commencement of treatment, at least one or two very

complete investigations of the cardiac sounds and impulse, to be used as testing observations for the future. I have also, at the same time, carefully investigated the habitual and the existing state of the cardiac function, and the history of any uneasy sensations or functional phenomena indicating disease, which may have been present at a former period. This done, and the heart being found free from suspicion, I have in future observations dwelt as lightly as possible upon the local examination of the heart, merely assuring myself, from time to time, that there was no very material change requiring more systematic attention.

If a murmur has been present on admission, it has not been too hastily assumed to be a new morbid process, unless accompanied by pain, or by the signs of effusion, or by marked tenderness on pressure, either over the costal cartilages or in the epigastrium. I have not thought it too much to wait for twenty-four or forty-eight hours, before commencing treatment, for corroborative evidence of the existence of true pericarditis in a doubtful case. Of course, careful watching, and examination at least twice a-day, has been the rule in all suspicious cases.

But in many cases of murmur, even when decidedly originating under observation, it has been my practice to look for something more than murmur as an indication for commencing the treatment addressed to an acute pericarditis; and this for two reasons. The alarm created by the announcement that the heart is wrong, is, in susceptible subjects, a serious objection to making this announcement on slight grounds. Having, accordingly, the clear conviction that many friction-murmurs, unaccompanied by signs of effusion, or by cardiac symptoms, may be safely neglected; and being further of opinion that, as a general rule, the treatment of the rheumatic condition of the system is the best treatment also for the pericarditis, I have been slow to give effect to anticipations of evil founded on the presence of a mere murmur, particularly if slight and narrowly circumscribed in locality. The immense majority of such murmurs are found to present themselves over a part of the right ventricle, between the third and the fifth costal cartilage; and I have rarely found them, except when accompanied by tenderness on pretty *firm* pressure, or by signs of effusion, or of marked excitement of the heart's action, or by a short dry cough, or by cardiac oppression and angina, give any cause for permanent uneasiness.

It is consistent with my observation, though opposed to the statements of several authorities, that one or other of the above-mentioned symptoms is rarely wanting when signs of effusion, even to a limited extent, are superadded to those of murmur. In fact, I more and more tend to disbelieve in *really acute pericarditis, apart from vital phenomena or symptoms;* although many cases called pericarditis may have been insidiously developed, and have escaped attention till a late period, either from inattention, or from their not being

really acute in the sense of demanding treatment. Among the symptoms mentioned, the dry short cough is perhaps the one most likely to attract attention, and should always lead to the suspicion of pericarditis, when not explained by the state of the lung. Moreover, the symptoms mentioned are occasionally developed as the earliest phenomena of the disease, being succeeded by the murmur within two or three days, as has been shown by Dr. Mayne, of Dublin, and Dr. Stokes.

With regard to the treatment of pericarditis actually pursued by me in hospital practice. I shall keep in view chiefly the rheumatic form, inasmuch as most of the others present little room for remarks tending to any satisfactory result.

In saying that rheumatic pericarditis will usually end favourably under various methods of treatment, I am very far from wishing to depreciate the value of medical practice in this disease. No doubt, the field within which our operations are to be restricted is more limited than it has been supposed to be by those who think that a particular method is a *specific* against inflammations in general, or pericardial inflammation in particular. But I trust that few educated members of the medical profession will think the worse of any treatment, because it does not claim the character of a specific method. What I argue is, not that treatment is of no use, but that the general and constitutional treatment applicable to rheumatism should over-rule the means addressed to the local disease, except within the very narrow limits which I shall presently point out.

I find it impossible to arrange the facts of my experience in this matter in the statistical form. For, in the first place, I do not know how many cases of really acute pericarditis I have treated; and in the present state of our knowledge, as indicated in a preceding part of this paper, it may safely be said, that every attempt to number such cases must involve elements of most serious fallacy. But, in the second place, I do not know how many of the cases I have treated have got well in the end, and looking to the remote consequences; some of them, for instance, may have recovered for the time with more or less of adhesion of the pericardium. Of this only I am certain, that as physician to the Royal Infirmary, I have not had under my care a case of rheumatic pericarditis fatal during the acute period of the attack.

Let me try to turn this fact to its proper use, as regards what may be called the heroic methods, bleeding and mercurial salivation.

I believe that the profuse employment of blood-letting in inflammations has long been diminishing, and that in Edinburgh, at least, it is pretty nearly extinct. I shall not, therefore, occupy space in demonstrating that that is a wrong thing which, to the best of my knowledge, very few indeed are in the habit of doing. For many years past, I have not heard that any physician in Edinburgh has used a lancet in rheumatic pericarditis. At all events, I am very sure

that the lancet is used most sparingly by the profession in general, and has been so for many years past.

But I am not so sure about mercury. Undoubtedly the use of this treacherous mineral is now marked by very great caution; and we almost never hear of those bad consequences which are the direct result of excessive mercurial action. But is its use, as some even now use it, expedient or necessary? And in rheumatic pericarditis in particular would patients recover better, or worse, were no mercury exhibited? Without altogether holding the question as decided, I am strongly inclined to answer both of these questions in a sense unfavourable to the use of mercury. My own use of this much vaunted and much abused remedy has been rather experimental than founded on conviction.

The reserve with which I have used a remedy which has so much testimony in its favour, may appear to require some explanation. The truth is, that, as a student, it was my fortune to serve in the hospital under a very bold mercuralist—a man of the most humane character, and of the most entire conscientiousness, who proved his sincerity in regard to this subject by undergoing, in his own person, three distinct salivations in the course of his fatal illness. Under this gentleman's directions I learned much that was valuable; and, among the rest, something in regard to the bad effects of mercury in rheumatic pericarditis. But I have never succeeded in learning anything as to its good effects, though on many occasions afterwards I have administered it with such caution as my knowledge of it inspired. It may be said that I have not done justice to the remedy. In one sense, this is true; for I have very rarely given it, except after other remedies. But surely, with a remedy of the power ascribed to this one, and especially regarded as promoting the absorption of exudation, it is no real injustice to call it into operation only in cases of a certain degree of severity, and to watch with care its influence upon cases that have in some degree resisted other treatment.

But whether I have done justice to mercury or not in my personal trials of it, I believe I have used it so as to do justice to nature, which is, after all, perhaps a better thing. For, assuredly, if I had adopted the plan of giving mercury instantly, in every case where a slight roughness existed in connection with the first sound of the heart, I should have failed to observe that the immense majority of these cases never went beyond a slight roughness, or, at most, a slight but decided friction-sound; that of those which went beyond this, and were accompanied by a degree of effusion, a large proportion had only moderate effusion; and that, even when considerable effusion was present, a good cure was still possible without mercury.

I will conclude by mentioning, as nearly as possible, what has been the usual course of treatment adopted in the cases here referred to, in so far as it has differed from that of acute rheumatism, or of the other primary disease. I trust I have not acted under any narrow

or bigoted feeling of opposition to established doctrines, any more than of empirical devotion to single remedies.

In the very beginning of some cases of pericarditis, where the pain was very marked, and especially where it had strongly the characters of angina, leeches have not unfrequently been applied in moderate numbers. From four to six leeches so applied, and followed by fomentations, have very commonly relieved the pain, and been followed rapidly by improvement. Where relief occurred, but was not complete, the application has sometimes been repeated. More commonly, one application has been all that I have thought requisite; and this only when strength and condition permitted, and when the symptoms had a certain degree of urgency. General blood-letting has not once been practised.

Fomentations, sometimes plain and sometimes medicated with opium, friction with camphorated and ioduretted liniments, and in obstinate cases the use of blisters, have been the chief local remedies besides leeches.

To conclude, I believe the principles of the safe treatment of pericarditis to be as follows:—1. To make large allowance for the insignificant and spontaneously healing class of cases revealed more by physical signs than by symptoms, and to regard them as demanding little active treatment; 2d. To consider rheumatic pericarditis in general as a disease susceptible, to a great extent, of cure under mild palliative local remedies and fitting constitutional treatment; 3d. To hold the constitutional treatment as subordinate to that of the disease with which the pericarditis is associated.—*Edin. Med. Journal, Jan.* 1861, *p.* 626.

16.—ON THE INFLUENCE OF OZONIZED COD-LIVER OIL ON THE PULSE.

By Dr. E. Symes Thompson.

[The attention of the Profession was first drawn to this subject by Dr. Theophilus Thompson, in a paper read before the Medico-Chirurgical Society some time since.]

He recorded the cases of about twenty patients at King's College Hospital, to whom the ozonized oil was administered. The usual dose was two drachms twice a-day. Scarcely any effect was observed from doses of one drachm. The influence of the oil on the pulse increased in proportion to the dose in which it was given, the effect of half-ounce doses, two or three times a day, being more marked than that of smaller quantities. The following table exhibits, as simply and concisely as possible, the results:—

Table showing the Changes of Pulse under Ozonized Cod-Liver Oil.

Name.	Sex.	Disease.	Time of Taking Oil.	Pulse.	
				Before.	After.
			Days.		
J. P.	M.	Phthisis, first stage.	7	116	98
J. S.	M.	Ditto.	7	120	108
W. B.	M.	Tuberculous larynx.	21	112	92
M. H.	F.	Phthisis, second stage.	7	80	80
E. A. W.	F.	Ditto.	14	140	112
C. H.	F.	Phthisis, first stage.	14	100	104
R. N.	M.	Phthisis, third stage.	7	150	116
E. D.	M.	Phthisis, second stage.	7	138	112
J. O'D.	M.	Emphysema.	4	104	86
M. M.	F.	Phthisis, second stage.	5	140	108
E. R.	F.	Anæmia.	4	120	116
M. S.	F.	Phthisis, third stage.	2	94	92
E. H.	F.	Phthisis, second stage.	10	104	88
W. S.	M.	Phthisis, first stage.	7	104	104
P. R.	M.	Phthisis, third stage.	4	104	96
S. E.	F.	Phthisis, second stage.	20	120	92
G. M.	M.	Phthisis, third stage.	4	140	120
H. C.	F.	Phthisis, first stage.	2	108	95
W. H.	M.	Phthisis, first stage.	5	120	96
J. R.	M.	Phthisis, first stage.	9	120	98

The author also recorded the effects of ozone in another form. He had made use of the ozonized water (as prepared by Condy), and found its influence in retarding the pulse considerable. In reference to a belief still prevalent amongst some members of the profession that ozone was a high oxide of hydrogen, the author remarked that he had used the peroxide of hydrogen (prepared by Messrs. Bullock), and found that it exerted no special influence on the pulse, which was more often accelerated than retarded under its use, which seemed to corroborate the conclusions of Dr. Andrews, who showed (in the "Philosophical Transactions of the Royal Society," vol. cxlvi. p. 1, *et seq.*) that ozone was not an oxide of hydrogen, but simply an allotropic modification of oxygen. Of the 20 cases in which ozonized oil was given, in eleven the pulse was reduced more than 20 beats a minute; in 4, upwards of ten beats; in 1, no effect was produced; and in 1 only was any permanent acceleration observed, and this could only be fully accounted for by disturbing circumstances. Of the 7 cases in which ozonized water was given, in 3 the pulse was lowered more than 20 beats, in 3 about 10 beats, and in 1 it was at one time retarded and at another accelerated. Dr. Thompson drew special attention to the

importance of the inquiry as connected with the treatment of phthisis, since, in this disease, anything that could retard the excessive rapidity of the vital changes would likewise check the development and progress of the disease. He showed that this had long been realised by the profession, and mentioned several remedies that had been used for this end, especially digitalis, which, though useful in some cases, was greatly inferior to ozone, being both less certain in its action, and often dangerous, from its cumulative tendency; while ozone exerted on the heart, not a depressing, but a strengthening and invigorating influence. It was suggested that the explanation of the remarkable effect of ozone in phthisis might be found in the greater affinity which phthisical blood had with oxygen—an affinity which it also preserved when in the allotropic form of ozone. Before concluding, the author alluded to a paper recently published by a French physician, "On the Use of Ozonized Oil of Turpentine in Hæmoptysis," in which the suggestions made by Dr. Theophilus Thompson, in 1859, had been followed, and observed that he had himself prescribed turpentine with ozonized cod-liver oil in hæmoptysis with much benefit.—*Med. Times and Gazette, March* 9, 1861, *p.* 266.

17.—ON FIBRINOUS DEPOSITS IN THE HEART.

By NATHANIEL CRISP, Esq., Swallowfield.

[The following is extracted from the 'Annual Retrospective Address,' delivered before the Reading Pathological Society. After detailing one or two other cases, in which fibrinous clots have been found in the heart, and had been the cause of death, the author proceeds:]

Uterine Phlebitis: Formation of Fibrinous Clots.—Mr. WALFORD read notes of the case and exhibited the uterus and uterine appendages of a woman, who had died of phlebitis. She was the mother of a large family, had been confined a month, and about this time was looking pale and ill. She complained of pain, tenderness and swelling, below Poupart's ligament; at the same time general constitutional disturbance existed. She continued in this state for six days, when increased præcordial dulness, and bellows-sound, existed over the cardiac region. No rheumatic taint or previous heart disease had existed. Rapid emaciation ensued; cough came on a day or two afterwards, and fluid blood was expectorated. Nine days before death, the right arm became swollen and very painful; and on the day prior to her decease, no pulse was to be detected at the wrist, and the jugular veins were swollen like a cord. Death took place on the eighteenth day of her illness, seven weeks after her confinement. The lochia continued in a slight degree the whole time. On removing the uterus and appendages, a granular kind of spot, of about the size of a shilling, was seen on the os. The lining membrane of the uterus was vascular. Some of the uterine veins were blocked up with fibrinous

clots, and this state could be traced into the body of the uterus; a coagulum existed in the right uterine vein, close to its entrance into the vena cava. Serous effusion was found in the abdominal and right pleural cavities, as well as within the pericardium. The liver was large and nutmeggy. The lungs were generally gorged with blood. The right side of the heart was filled with clotted blood, and a fibrinous clot extended from the auricle into the ventricle; and in the right ventricle was a layer of fibrine of a rose colour. The tricuspid valve was a little thickened; and one of the larger divisions of the pulmonary artery was completely blocked, as well as the jugular and subclavian veins on the right side. Mr. Walford, in his remarks on this case, drew attention to the length of time which had elapsed between labour and death, and was inclined to think that the granular appearance on the os uteri, (already referred to), was an ulcer, whence all the mischief sprang, and concluded by making some remarks on the utility of keeping the vagina well syringed in such cases. The general opinion of the meeting appeared to be against the idea that a breach of surface existed; that, if any ulceration had existed, it was not of recent date, and was healed,

Remarks on Fibrinous Deposits.—The subject of fibrinous depositions in the heart, of *ante mortem* origin especially, has of late been brought more directly under our notice by Dr. Richardson's most able and interesting publications; and as a body, we are much indebted to him for so clearly explaining cases of death, which previously, to a certain extent, appeared obscure. Although, as yet, such discoveries may have done but little to lessen mortality, still, to use his own words, they "may make prognosis more accurate, may guard us against false practice, and in time, may lead to suggestions, bearing directly on curative treatment;" and, doubtless, to some extent, his words have already proved true. Is it not much more satisfactory to ourselves to be able to make up our minds as to the true nature of our patients' symptoms, than to be merely certain that a fatal issue is about to ensue; when such an event has taken place, and a *post mortem* examination reveals the true cause, to be satisfied that no remedies we have used could have accelerated it, and perhaps to feel now and then justified in cherishing the hope that our efforts had at least the tendency to prolong existence, if not to lead to a still more happy result? We may not have been guilty of fancying that we had discovered a serpent in the blood, as some of our forefathers did; but I suspect that many of us have allowed the fact of the existence of cardiac clots to pass by unnoticed, and have ascribed death to more remote causes. At the same time we must not be too ready to attribute all cases to such a cause; even if the symptoms are much allied to those described as indicative of such formations. In proof of this, I may mention a case occurring in the practice of a friend of mine. A gentleman, during a short illness of only twenty-four hours, exhibited

symptoms which might have led to such a supposition. On examining the body after death, a considerable quantity of blood was found in the pericardium; no rupture of any portion of the heart or large vessels could be detected; the blood appeared to have simply oozed through the muscular structure of the ventricle. This case is, I think, as rare as interesting, and deserving of mention in connection with the present subject.—*Brit. Med. Journal, Nov.* 24, 1860, *p.* 918.

DISEASES OF THE ORGANS OF RESPIRATION.

18.—ON THE USE OF LOOPED WIRE IN THE REMOVAL OF FOREIGN BODIES FROM THE AIR PASSAGES.

By Dr. J. J. Tomson, Davenport, Iowa.

[The patient was a lad about eight years of age, who had accidentally got into his trachea a piece of clay pipe-stem, about one and a half inches long, and of large size. Tracheatomy was performed, but the piece of pipe could not be seized by any expedient tried.]

After trying the forceps, hooks, &c., I suggested the use of a *looped wire.* A piece of small wire, about two feet long, was obtained, and looped in the middle, of sufficient size to embrace the end of the pipe-stem (on the same principle as removing corks from a bottle with a string). The patient's head being well thrown back, I proceeded to introduce the *looped wire.* On passing it down to the right bronchus, it came in contact with the foreign body. At this point, I raised the end which I held in my hand, and pressed the end next the foreign body towards the spine, so as to pass my wire behind the pipe-stem. The pipe-stem was firmly impacted in the bronchus, so that it required some force to push the wire between it and the wall of the bronchial tube. The wire was passed some two inches or more below the point of obstruction, and then, on gently withdrawing it, the loop came in contact with the lower end of the pipe-stem, which was thus easily removed. The orifice of the trachea was closed, and the boy made a rapid recovery.

On the twenty-fifth of last month, my partner, Dr. Maxwell, and myself, were sent for by Dr. Carpenter of Blue-Grass, to assist him in removing a grain of corn from the trachea of a child about one year old. The operation was performed by Dr. Maxwell; after which, I passed the loop of wire as in the other case. It was passed down the right bronchus, and passed quite easily the point of obstruction; and on its removal, it brought the kernel of corn into the trachea, which soon after made its appearance at the orifie, and was easily removed.

I wish to call the attention of the Profession to this *simple, cheap*, and *harmless* instrument, from the fact that I believe it will succeed in some cases where nothing else will. It can be used with perfect freedom by any one who is acquainted with the anatomy of the lungs, in searching far into the air-passages for small bodies, with little or no risk of producing serious irritation. There are other cogent reasons for its trial which will suggest themselves to the mind of every medical man. I submit its trial, with the cases above reported, to the Profession, hoping that it may be found of *some* service in such painful and unfortunate cases.—*American Med. Journal, Jan.* 12, 1861, *p.* 25.

19.—*On the Palliative Treatment of Asthma.*—By T. L. PRIDHAM, Esq., Bideford, North Devon.—The first on the list is stramonium, the fumes of which may be collected in an inverted glass bowl with a narrow mouth; the bowl being charged to its full is placed under the mouth of the patient, who is directed to inhale to the fullest extent in his power the smoke which has been collected in the bowl, taking care to hold his head away from the bowl when an expiration takes place. Chloroform, both taken internally or inhaled, is a powerful remedy, but it must be employed with caution, and never administered except by a medical attendant. The fumes of nitre paper in a state of ignition, well inhaled, is often a valuable remedy. Care should be taken to procure the best prepared from a good chemist. Chloric ether and the tincture of the lobelia inflata will occasionally relieve. Bicarbonate of soda, as well as chlorate of potass, given in full doses, I have frequently seen produce a good effect. Again, I have seen repeated doses of sulphate of alum procure relief, the powder being allowed to dissolve on the tongue before it is swallowed, in ten grain doses. I have also seen the fumes of tobacco, inhaled as I have recommended in the use of stramonium, relieve, when other remedies have failed; but I do not like this remedy, it produces such deadly faintness and nausea. Small drinks of the best Mocha coffee, made strong, will often procure relief. On two occasions, when every other remedy failed, I succeeded in procuring almost instant relief, by injecting two grains of morphine and a drachm of tincture of assafœtida. These were cases where mental distress appeared to be the exciting cause.

I have often sat at the bedside of one, suffering from the severest form of the disease, watching with great anxiety the result of prescribed remedies, and it has not unfrequently happened that many have been tried without relief, the patient all this time gasping for life with sufferings the most intense, when relief has at length come from a remedy apparently the most unlikely to procure it—so capricious is the disease, and so uncertain the remedy in asthma cases of this particular character.—*Brit. Med. Journal, Dec.* 29, 1860, *p.* 1009.

20.—DRILL FOR AUSCULTATION.

By Dr. THOMAS K. CHAMBERS, Physician to St. Mary's Hospital.

[Students should shelve long books on auscultation and study nature by using the following "order of drill." The student will, in three or four days, acquire all that is practically useful in the clinical examination of the lungs.]

FIRST STEP of Drill.—Look over the whole outside of the chest and see if there be anything wrong about the SKIN or deformity of SHAPE. Note particularly *flatness* or *bulging* of ribs. In males this is quickest done by stripping the upper part of the body all at once. A female's shift is looser, and can be drawn *first backwards* so as to look down the back and sides, *then forwards* to look at the front and sides.

SECOND STEP.—Get behind the patient, put your two thumbs on the top of the two scapulæ, and lay your fingers firm and flat on the collar-bones and upper ribs. Make the patient sigh deeply, and then looking along your fingers you can compare the motion of the two UPPER LOBES of the lungs with one another. If a bright light shines on your finger-nails you can detect a difference in expansion of one-twentieth of an inch. Then put your thumb-tips on the lowest dorsal vertebræ and span the waist as far as you can, make the patient sigh, and you feel the extent of motion of the LOWER LOBES.

THIRD STEP.—Percussion—what do you want to find out by it? Only if one part of the pulmonary tissue is more solid than another. Of course it cannot be *all* solid; so if any part is so, it will contain less air, and sound therefore less drummy, less "resonant" than a more healthy part with which you compare it. Do not think about finding anything else, or you will not pay due attention to this.

Place the patient so that you can lay the fingers of your left hand flat on the thorax. Swing the right hand free and easy, and hammer with the finger-*tips*, on your knuckles in the following order:—

Auscultation Posts.

1. On right clavicle.	2. On left clavicle.
3. Under right clavicle.	4. Under left clavicle.
5. Above right scapula.	6. Above left scapula.
7. Close under right scapula.	8. Close under left scapula.
9. Under right mamma (an inch below).	10. Under left mamma.
11. Right lateral region.	12. Left lateral region.

Do not go on hammering long, but compare by two or three quick strokes each place with the same place on the other side. In all these parts you ought to find the resonance of the two sides equal except in the left mammary region, where the heart ought to make dull the spot it is felt to beat in.

If you can detect no absence of normal resonance, take a note of it, and go on to next step. But if there be dulness where it ought not to be, percuss round and round the spot where you first find it, and take a note of the EXTENT OF DULNESS.

FOURTH STEP.—Apply your stethoscope *flat, to the naked skin.* Apply it to the same posts in the same order that you have percussed, and make notes in the same order of what you hear. Do not trust to what you may consider deviations from an ideal standard, but compare the two sides and note if they differ.

Now, as to what you may expect to hear:—

Natural sounds.—Some healthy lungs breathe very softly, some very harshly; so the degree of sound, if equal throughout, tells nothing. Note therefore only when they are *defective* in any places, while breathing fully elsewhere.

Morbid sounds.—Think first what your ear *can* tell you, and do not trouble your brains by thinking about what it *cannot.* It can tell you—

(*Point* 1) Whether air enters the subjacent lung or not;

(*Point* 2) Whether it passes through natural, soft, yielding tubes, or through stiffened tubes;

(*Point* 3) Whether it bubbles through fluid or not;

(*Point* 4) Whether the bubbles are large or small, and therefore whether the open space they break in are large or small.

As to Point 1—if no air enters, there is an end of your observation at that spot; take a note and go on.

As to Point 2—if the tubes and tissues are natural, the sound of expiration is less than the sound of inspiration. If it is equal to it or greater than it at any spot compared with the opposite side, call it "tubular breathing"—that is to say, the air goes through tubes only, and does not enter into the terminal vesicles. The typical example of it is what you hear on each side of the sternum over the large bronchi, and therefore it is often called "bronchial breathing." Tubular breathing tells you that the tubes are stiffened either by their coats being swelled or hardened, or by the surrounding tissue being condensed.

As to Point 3—air passing through a thickish fluid, like mucus or pus is sure to make a crackling noise. If it does not do so, if there are continuous whistling, snoring, or piping sounds, they may be safely called "*dry*" sounds, and noted down as "whistling," "snoring," or "piping," without waiting to find a French name for them.

As to Point 4—the size of the crackles is important. The very finest are evidence of their being situated in the terminal vesicles of the lung. Imitate this fine crepitation by rubbing your hair between your fingers, and then make as large a bubble as you can with saliva between your lips, and you hear the types of the two ends of the scale of size. The larger they are, the larger the bronchus or cavity in which the bubbles break.

I said that air passing through a viscid fluid is sure to make a crackling noise, so that the *absence* of crackling proves the absence of fluid. But the *presence* of crackling does not necessarily prove the presence of fluid. Unfortunately animal membranes will make a creaking just as leather does. And the creaking of inflamed pleura exactly resembles the crackling of small bubbles. *The sounds themselves* are indistinguishable. Do not be taken in by persons who profess to distinguish them; they do it by collateral circumstances, as you must do. When you hear crackling, put it down in your notebook, and leave the distinction to the future.

FIFTH STEP.—Go round again to the same posts with your stethoscope, at each post making the patient speak a few words of several syllables—such as counting on from twenty. What you have to notice is—(1), whether the voice is heard through the stethoscope at all; (2), if so, whether it is (3) *natural*, that is, a confused sort of buzzing; or (4), *with increased vibration*. In cases where the vibration is increased notice if it is like a man talking Punch—i. e., *ægophonic;* or if, when the patient whispers, you can hear the whispering up the stethoscope—i. e., *whispering pectoriloquy*.

All besides this is fancy work, showy, but waste of time. Even on this real work do not pause too long; the moment you hear a sound, go on to the next post, or you confuse your ear and learn to dawdle.

It is a great saving of time in practice to be able to take notes with rapidity, as well as to auscultate with rapidity. For this purpose I recommend you to acquire the habit of drawing a rough but correct outline of the chest and shoulders, and to have certain marks to record what you have observed. To do it occupies not a quarter of the time of a written description. I can make the two outlines of the front and back of the chest, on the slate in fifty seconds, and in thirty seconds more I can mark upon each of the posts of auscultation what I hear by these or any other arbitrary signs.

In this sketch the most commonly observed phenomena are recorded by conventional signs, the less common by the first few letters of their names, the still less common by the name written at full length.

A curved line along the spine,), may denote that the spine is curved to one side or another; and another that there is bulging of the ribs.

Oblique lines, ///, that there is flatness of the chest observed in the first step of the drill.

Parallel lines, =, that there is dulness on percussion.

T, or *t*, that there is tubular breathing; the loudness and depth of tone, or smallness and sharpness of tone, being indicated by the size of the letter, showing the size of area it is made in.

C, or *c*, or *cc*, that there is crackling large or small, according to

the size of the letter, and extensive according to the number of letters.

B may denote bronchial or vibratory voice.

P, pectoriloquy.

Def. m. may be written on a line to denote *deficient motion* of the ribs as far as that line extends.

Æg., may denote *ægophony; res.*, *unnatural resonance*, with a line drawn round it to show how far it extends; while "*whispering voice*," "*metallic tinkling*," "*cracked pottery*," or any fancy sound which you think it worth while to record, had better be written at full length, with a dotted line leading from the post where it is heard.

H of course means *healthy sounds*, which you should take a note of, though they do not necessarily prove that the lung is healthy. Remember, now and always, that your note records what your hear, not your diagnosis. Thus, on the diagram you may see I have noted in the left infra-clavicular region the ribs are flattened, though the clavicle has not fallen in; that there is a lateral curvature of the spine, with the greatest projection towards the left side; that the ribs on the left side bulge considerably; that there is deficient motion of both upper and lower lobes of the lungs on the left side; that there is dulness on percussion in the left infra-clavicular, left suprascapular, left infra-scapular, and left lateral regions; that the cardiac region is resonant; that there are crackling or crepitant râles in the right supra-clavicular and left supra-clavicular regions; that in the latter these are the larger and coarser; that there are the same râles in greater amount and with small tubular breathing in the right infra-clavicular; that in the left infra-clavicular there is also very loud tubular breathing, large and small crepitant râles, very loud, bronchial voice, whispering pectoriloquy, and metallic tinkling; that in the left infra-scapular region there is ægophony; that in the left lateral there is bronchial voice and mixed crepitant râles of various sizes; that the sounds were those of a healthy condition of lung in the right infra-scapular, infra-mammary, and lateral regions.

I need not ask you which mode of record you think will take longest or be most exact.

If you are not quick in drawing with a pen, a further saving of time may be effected by having the outline cut in wood for a few shillings, and stamping it in your case-book when and where required. Or you may have it stamped on thin paper, gummed at the back like a postage stamp, and stick it in when wanted.

Under any circumstances, whether you adopt my method or one of your own, I think it of paramount importance that you should take notes, not of your diagnosis, but of the *grounds on which you base it.* It is of no use to yourself, your patient, or to science to remember that on such a day you thought that there was pneumonia or tubercle, but it is of great use to all to remember why you thought so.—*Lancet, March* 30, *and April* 6, 1861, *pp.* 307, 334.

DISEASES OF THE ORGANS OF DIGESTION.

21.—ON THE COMPARATIVE DIGESTIBILITY OF VARIOUS ARTICLES OF DIET.

By Dr. GEORGE E. DAY, Professor of Medicine in the University of St. Andrews.

[The following, like several other short articles in this volume, is extracted from Dr. Day's recent admirable work on 'Chemistry in its Relations to Physiology and Medicine.']

A chapter on "The Digestion," would obviously be imperfect if it included no notice of the digestibility of the different ordinary articles of food. Unfortunately, our knowledge on this subject is very imperfect, the experiments which have been made being in part unsatisfactory in the mode in which they were devised, and in part, giving absolutely contradictory results. The experiments of Gosse, who possessed the power of vomiting at will, and who thus brought up food which had remained for different lengths of time in the stomach, and even the much more valuable observations of Beaumont on Alexis St. Martin, are unsatisfactory in the following respects. (1.) These observers regarded the digestive process as ended when the food in the stomach had been reduced to a uniform pulp, and altogether neglected the changes impressed upon the food beyond the stomach. (2.) They used mixed, variously prepared, often half vegetable and half animal food, a method which is totally unfit for determining the digestibility of individual articles of diet. (3.) No mention is made, at least in Beaumont's experiments, of the quantity of food that was taken, or of how minutely it was divided. Without entering into details of the discrepant observations made on this subject by Schultz (who at a certain time, after feeding dogs and cats with different kinds of food, killed them and examined the contents of their stomachs), and by several other physiologists, it is sufficient to quote the opinion of Blondlot, who first introduced into physiology the operation of artificial gastric fistula, and who obtained such very indefinite and inconclusive results, that he was led to express the view that the digestibility of different articles of diet depended solely on the state of the stomach at the time of the experiment, and that it is pure waste of time to labour at the determination of the digestibility of individual articles of food. Busch found in his case of intestinal fistula that flesh, eggs, and vegetables, began to appear at the fistulous opening in from fifteen to thirty minutes after they were swallowed, but that if a copious meal had been taken, traces of these substances continued to appear for three or four hours. He gives the following results:—

Boiled eggs appeared in 3 experiments after 26, 20, and 35 minutes.
Cabbage „ in 2 „ after 19 and 15 minutes.
Flesh „ in 2 „ after 30 and 22 minutes.
Parsneps „ after 12 minutes.
Potatoes „ after 15 minutes.

The following table, extracted from Busch's memoir, is deserving of attentive study, as showing how much of each of the articles of food mentioned in the first column was absorbed before reaching the fistula in the jejunum.

	Ratio of weight of food to weight of matters subsequently discharged through the fistula.	Ratio of solid constituents of food to solid constituents of the matters discharged through the fistula.	Ratio of solid constituents of undissolved portion of discharged matters to the solid constituents of the dissolved matters.
Gelatin	1 : 3·675	1 : 0·94	
Boiled eggs	1 : 2·73	1 : 0·76	1 : 2·3
Flesh	1 : 1·73	1 : 0·35	1 : 2·27
Milk	1 : 1·25	1 : 0·62	1 : 4·3
Parsneps	1 : 1·2	1 : 0·49	1 : 0·94
Cabbage	1 : 0·91	1 : 0·58	1 : 0·66
Potato soup	1 : 0·7	1 : 0·53	1 : 1·5

From the experiments on which these numerical results are based, Busch concludes that flesh is more digestible than eggs, that parsneps are more digestible than potatoes or cabbage, and potatoes more digestible than cabbage.—*Glasgow Med. Journal, Jan.* 1861, *p.* 468.

22.—ON THE INTESTINAL CONTENTS.

By Dr. George E. Day, Professor of Medicine in the University of St. Andrews.

The bright yellow semifluid excrements of infants at the breast contain, as was shown by Simon, a large quantity of fat, a considerable amount of coagulated bile, undigested casein, and a sufficient quantity both of the biliary acids and bile-pigment to give certain evidence of the presence of these bodies by Pettenkofer's test and by nitric acid respectively. Epithelial structures are moreover present.

The excrements sometimes present a green colour, which, until comparatively recently, was regarded as a sign of the presence of an excess of bile. This is, however, seldom the cause of a green colouration of the fæces, and only occurs when there is at the same time an excess of bile and a preponderance of free acid in the intestine; as, for instance, in *icterus neonatorum*. In these cases the ordinary brown bile-pigment seems to be converted into the modification termed bilifulvin. As we shall presently show, the presence of blood may sometimes give rise to a green colour.

Lehmann has confirmed the observations of Höfle and others,

regarding the occurrence of sulphide (sulphuret) of mercury in the green or greenish-black stools that are voided after the use of calomel. The colour in this case seems due partly to the presence of almost unchanged bile, and partly to that of the mercurial compound.

The excrements often assume a black or dark-green colour after the prolonged use of ferruginous preparations or chalybeate waters (especially such as contain sulphate of soda with carbonate of protoxide of iron). Lehmann has definitely shown, from analyses of the green and black excrements of persons taking the Marienbad waters, that the colour is here due to the presence of protosulphide of iron.

The excrements are usually green after the medicinal use of indigo, and are often black after charcoal has been taken.

As only a definite quantity of fat can be absorbed by the intestines in a given time, food very rich in fat or the ingestion of cod-liver or other oils may give rise to the presence of a large quantity of fat in the excrements. In some diseases, especially such as specially interfere with the general nutritive processes, such as pulmonary phthisis, Bright's disease, diabetes mellitus, and more particularly diseased conditions of the pancreas, an augmentation of the fat in the fæces is often observed.

Sugar has been occasionally found, but it is not always present, in the excrements of diabetic patients.

The occurrence of blood in the fæces is by no means rare. Omitting all notice of those cases in which its presence is too obvious to be overlooked, we may remark that when the hemorrhage is very slight, and proceeds from the stomach or small intestine, it may impress upon the fæces a peculiar colour and appearance, whose cause may easily escape recognition. In such cases we often have black or chocolate-coloured tar-like stools, in which imperfect blood corpuscles can be discovered by the microscope, and in which hæmatin can be chemically detected. In some of the intestinal diseases of young children, and occasionally in some forms of continued fever, semi-fluid green excrements are discharged which owe their colour to a slight admixture of blood which may be readily detected by the microscope.

Albumen in a coagulable state sometimes occurs in normal excrements; it is, however, found in large quantity in the evacuations in dysentery and in typhus (and not unfrequently in the fluid or semi-fluid stools which sometimes occur in Bright's disease), and in lesser quantity in cholera.

Epithelial structures occur in the stools in all cases of diarrhœa. They are extremely abundant in the evacuations in cholera.

Cytoïd curpuscles are abundant in the evacuations in intestinal catarrh and in dysentery; and are occasionally observed in typhus and cholera. The glassy mucus which sometimes occurs in roundish masses in catarrhal affections of the large intestines is apparently secreted by the follicles of the colon.

The intestinal evacuations have been especially studied in typhus and cholera.

In typhus the stools are usually fluid, of a yellowish brown colour, an abominable smell, and an alkaline reaction. On standing for some time they separate into a yellowish sediment, consisting of flakes of undigested food, white granules of about the size of a pin's head and and probably resulting from intestinal ulcers, epithelium, and often mucus, and numerous crystals of phosphate of ammonia and magnesia; and an opaque supernatant fluid of a yellowish or pale brown tint, generally containing albumen and a large quantity of chloride of sodium.

In cholera the main peculiarities of the stools are, in addition to the abundance of epithelium already referred to, an extraordinary quantity of water, a little albumen, very little biliary matter, and a large amount of salts, among which the chloride of sodium preponderates to such an extent as often to exceed in amount all the organic matters. These evacuations contain only from 1·2 to 2·4% of solid constituents. The addition of nitric acid gives rise to a rose-red tint in these stools, which is often also observed in the evacuations in typhus.—*Glasgow Med. Jour.*, *Jan.* 1861, *p.* 465.

23.—ON THE ACTION OF TOBACCO, TEA, AND COFFEE.

By Dr. GEORGE E. DAY, Professor of Physiology in the University of St. Andrews.

With regard to tobacco Dr. Hammond made two sets of experiments—(1.) when a sufficiency of food was taken to keep up the weight and vigour of the body, and (2.) when an insufficiency of food was taken. It should be mentioned that he is not in the habit of using tobacco in any form, and that during these experiments he smoked 450 grains daily. He finds:—

1. That tobacco does not materially affect the excretion of carbonic acid through the lungs, but that it lessens the amount of aqueous vapour.

2. That it diminishes the amount of fæces.

3. That it lessens the quantity of the urine and the amount of its urea and chlorine, but that it increases the amount of free acid, and of the uric, phosphoric, and sulphuric acids.

These results differ in several essential points from those yielded by alcohol. The fact that the amount of carbonic acid was not diminished, would indicate that the consumption of the fat of the body is not lessened by the use of tobacco. The general metamorphosis of the tissues would seem to be retarded, seeing that both the urea and the chlorine of the urine are diminished (in the first set of experiments the mean daily diminution of urea and of chlorine was represented by 42·4 and 23·0 grains; while in the second set the corresponding

numbers were 62·5 and 15·0 grains): but as the phosphoric and sulphuric acids are increased (in the first set the mean increase being 24·2 and 4·4 grains, and in the second set 30·2 and 8·3 grains), we can only explain the apparent inconsistency in these results by assuming that there is an increased oxidation of the phosphorus and sulphur of the brain and nervous tissue generally, although the metamorphosis of the other nitrogenous tissues is lessened.

Tobacco resembles alcohol in these respects, that when the food is sufficient to preserve the weight of the body it increases that weight, and when the food is not sufficient and the body in consequence loses weight, it restrains that loss; but it differs from alcohol in being unattended with any unpleasant effects upon the circulatory system, though its action on the brain is apparent in increased nervous excitement, followed by a pleasant feeling of ease and contentment.

Tea and coffee are usually believed to have a somewhat similar effect to that which, as we have shown, is produced by alcohol and tobacco.

Bocker made a careful series of observations upon himself, for the purpose of testing the effects of tea on the organism.

Three series of experiments were made under varying conditions, but they all coincided in the following particulars:—

1. Tea in ordinary doses has not any effect on the amount of carbonic acid expired, the frequency of the respirations, or of the pulse.

2. When the diet is insufficient, tea very considerably limits the loss of weight thereby entailed.

3. When the diet is sufficient the body is more likely to gain weight when tea is taken than when not.

4. Tea very much diminishes the loss of substance in the shape of urea.

5. Tea very much lessens the quantity of fæces.

6. The loss by perspiration is also limited by tea.

Dr. Edward Smith has recently arrived at certain conclusions which are the very reverse of those of Bocker. He maintains that tea increases the amount of carbonic acid (and here he is likewise opposed to other observers), and that it actually accelerates the waste of the tissues. It is to be trusted that this discrepancy of opinion between two good observers, may lead to this important question being further examined and definitely settled.

The action of coffee on the organism has been carefully studied by Dr. Julius Lehmann. He has investigated the separate actions of the different constituents of the coffee-bean, viz., caffeine (identical with theeine) and empyreumatic oil, as well as of their mixture. The point that mainly concerns us in reference to the present subject, is (1.) that coffee protracts the decomposition of the tissues, and (2.) that the protraction is chiefly caused by the empyreumatic oil, and that the caffeine only causes it when it is taken in larger quantities than usual.—*Glasgow Med. Journal*, *Jan.* 1861, *p.* 470.

24.—*On Oxalate of Cerium in Gastric Affections.*—Dr. Charles Lee has tested the efficacy of oxalate of cerium, recommended by Professor Simpson of Edinburgh, as a remedy in gastric affections.

Dr. Lee first used the medicine in cases of obstinate vomiting in advanced pregnancy, which had resisted all the ordinary remedies, such as creosote, hydrocyanic acid, ice, bismuth, &c. In each of four cases of the kind, which he relates, he gave one or two grains of the oxalate of cerium every two or three hours with marked success; the vomiting being completely arrested. He has also found the effects of the remedy no less encouraging in the vomiting that often accompanies phthisis, in pyrosis, in hysterical emesis, and in various dyspeptic conditions, especially in atonic dyspepsia. In fourteen cases of the latter disorder, the effect was perceptible in a short time, both in relieving nausea, and in restoring the appetite. Dr. Lee confirms Dr. Simpson's statements as to the rapidity of action of the oxalate of cerium; but is disposed to question its action as a *sedative tonic*, as he has tried it without success in several cases of acute and subacute gastritis, both idiopathic and supervening on debauch, or delirium tremens.—*Amer. Journal of Med Science.—Brit. Med. Journal, Jan.* 5, 1861, *p.* 24.

25.—IS MERCURY AN HEPATIC SPECIFIC?

By Dr. Thudichum.

[This question has been broached by various authors lately. Dr. Thudichum and Dr. Inman have both written before on the subject. The following propositions express Dr. Thudichum's ideas on the subject; they agree essentially with Dr. Inman's.]

1st. That mercury does not make its appearance in the bile when given in the form of calomel has been proved by Mosler, ('Virchow's Archiv.' vol. xiii. p. 29.) Doses of from twenty-two to fifty-two grains produced no appearance of mercury in the bile, neither was the quantity of bile secreted thereby augmented. If I recollect right, an author in India, writing in the 'Lancet,' attempted to controvert the facts of Mosler, and stated that he had found mercury in the bile. Even if that should be so in some cases, there remains the fact, nevertheless, that the quantity of bile is not increased by that agent.

2nd. H. Nasse, Kölliker, and H. Müller found that the addition of calomel to food, which under ordinary circumstances produced a certain and normal quantity of bile in dogs, diminished the quantity of bile.

3rd. Taking into consideration three experiments of Kölliker and four of Scott's, related in Beale's 'Archives,' Dr. Inman comes to the conclusion, that the chances are six to one that calomel will diminish the hepatic secretion. The facts I have stated make the proportions worse. In nine cases out of ten, calomel, whether it purges or not, will

diminish the quantity of bile secreted. Where it acts as a purgative, the diminution is most conspicuous.

4th. In my reply to the observations of Drs. Leared and Routh, I allowed that mercurials, particularly calomel, by their purging action, might relieve some disorders of the liver or other organs, or of the general system. This effect could be purchased at less cost by other purgatives, because the specific action of mercury was always hurtful, excepting, of course, in cases of syphilis. The mere fact, therefore, of cases of liver disease having been improved by the action of mercurials, is no proof of their specific advantage.

5th. The stools which are passed after the use of purging mercurials, particularly calomel, are supposed to contain more bile than usual. This assumption rests upon the observation that these stools are mostly green—a fact which appears to me to be at the bottom of the entire tissue of errors.

The green colour of calomel stools is due to sub-sulphide of mercury, just as the black colour of stools following the use of preparations of iron is due to sub-sulphide of iron. The sub-sulphide of mercury can be easily obtained from those stools by levigation, or chemical proceedings. So much is proved. On the contrary, it is not proved that calomel stools contain any increased quantity of either bile or biliary colouring matter (cholochrome.) From the experiments quoted above, the reverse of this unproved hypothesis will probably be found to be true—namely, that the green calomel stools contain a smaller amount of biliary colouring matter than an equal amount of solid matter from healthy fæces.

Note.—When the fæces passed after a dose of calomel or blue pill become green, they also begin to scald the anus. This sensation has been ascribed to the bile, which was then believed to be passing. It is only reasonable to ascribe this scalding, not to bile, which is not present, but to the sub-sulphide of mercury, which has been proved to be present.

6th. Dr. Inman adopts the estimate of the quantity of bile discharged by a man of 160 lbs. weight, in twenty-four hours as sixty-six ounces. The most accurate calculations from experiments upon animals permit me to assume that an adult person secretes between 195 and 675 grains of solid matter through the biliary channels. The amount of solid matter contained in human bile varies between eight and sixteen per cent. It therefore follows that the amount of average bile secreted by an adult in twenty-four hours fluctuates between 1200 and 9000 grains; or 2 oz. 240 grs., and 18 oz. 360 grs.—a vast difference from 66 oz. But substituting these figures for those given by Dr. Inman, and assuming the fæces to weigh half a pound instead of a pound, (the dry residue of a healthy man's fæces does not usually exceed two ounces), Dr. Inman's subsequent conclusions become still more correct.

7th. Dr. Inman assumes that the clayey, white stools of persons

suffering from jaundice, might contain the ingredients of bile minus only the colouring matter, This has not been proved to obtain. As, on the other hand, it has been proved that the healthy fæces contain no biliary matter except a derivate of cholochrome, Dr. Inman's assumption becomes very improbable, and the reverse opinion gains ground, that the clayey. white stools of the jaundiced contain neither bile, acids. nor cholochrome. I am far from admitting, as a reliable fact, that the liver may secrete colourless bile. I also cannot admit that in jaundice the colourless part of the bile could go into the intestines, while the coloured part passes into the blood. With regard to these points, I differ entirely from Dr. Inman, not because I think that such might not occur, but simply because no such occurrence has been proved.

8th. I take this opportunity to point out that the question of the discharge of any modified biliary matter besides cholochrome in the fæces requires further study. The ordinary biliary salts are certainly not present; the fæces contain little soda. But some modified cholic acid might still be contained, and escape observation, as it had hitherto done in gall-stones. According to Berzelius, five ounces of fresh excrements contained twenty-one grains of a matter similar to bile. Considering that 675 grains is the maximum, and 195 grains the minimum of solid bile, any modified cholic acid in the fæces could not be less than three, nor more than ten per cent. of the bile secreted in twenty-four hours.—*Lancet, Oct.* 27, 1860, *p.* 411.

26.—THE ACTION OF MERCURY ON THE LIVER.

By Dr. Thomas Inman, Liverpool.

[The question which Dr. Inman endeavours to answer in the following paper, is, whether the brown colour of the stools really is owing to biliary matter. The subject first suggested itself from several papers by Dr. Ward and Sir H. Cooper, in which it is taken for granted that such is the case.]

With an honest desire to attain exactness, I attempted to supply the required links, and made the inquiry, Upon what does the brown tinge in the stools depend? Dr. Ward gives his own case to show that diarrhœa with clayey stools may exist for weeks without there being any evidence of real hepatic disorder: in that observation I concur. I have already noted the persistence of white stools when an individual is on a milk diet. I know one case of severe headache always accompanied with clayey stools, in which mercurials *invariably make the stools whiter;* and another, in which the motions become brown again in four days without any medicine at all: clayey stools, therefore, do not necessarily indicate hepatic disease requiring medical interference. But Dr. Ward says, that mercury, as a general rule, restores the healthy colour sooner than any other drug,—granted, for

the sake of argument; but how does that affect the question if we rejoin, *the brown colour of the stools is due to a secretion from the colon?*

In cogitating over this matter, I was greatly struck with the acuteness with which Dr. Thudichum had struck what I felt was *the* weak point in my first paper. His remarks, as reported in the Journal, Oct. 27th, 1860, *a propos* of my notion, that the clayey stools might contain colourless bile, were, "This has not been proved to obtain; as, on the other hand, *it has been proved that the healthy fæces contain no biliary matter, except a derivate of cholochrome,* Dr. Inman's assumption becomes very improbable; and the reverse opinion gains ground, that the *clayey white stools* of the jaundiced contain *neither bile acids nor cholochrome.*" Taking this in its entirety, we assume that there is no direct proof that clayey stools contain *any bile.* But memory recalls many cases of jaundice with total suppression of bile, when the stools *have been brown.* Whence, then, the colour? This involves a very simple question, viz.: *Where does the brown colour of the stools begin?* That I might not deceive myself on this point, I propounded the question separately, without giving any reason for it, to four different young medical men, all close observers, and diligent in their attendance in the dead-house; the answer was the same from all, viz., *in the colon.* My own memory upon this point may possibly be treacherous, I therefore make the remarks interrogatively rather than affirmatively, and ask, "Did ever any one see the contents of the small intestines of a brown, deep yellow, or even a bilious hue? Did ever any one see, in cases of hernia, a brown fæcal matter flow from the bowel, if the small intestine only was implicated? Is it not a fact, that the intestinal secretion is always whitish prior to its reaching the ileo-cæcal valve, and that it attains its fæcal character and colour *in the colon? May not a clayey diarrhœa, then, simply demonstrate that the colon, and not the liver is sluggish?* I next examined with care the intestines of a man who had died in the Royal Infirmary rather suddenly. The canal was slit up from end to end, and it was clear that "fæcification" commenced in the colon: the direct evidence was against the bile being the cause of the colon. After this, I recollected Dr. Petrie's assertion, that the green colour of the stools in children who had taken calomel was due to a vitiated secretion from the bowels, and chiefly from the colon; and on turning to a private letter from my friend, Dr. Russell, of Birmingham, date, Nov. 3rd, 1860, I found the remark. "My strong suspicion—I dare hardly yet call it a belief—is, that what is called bile in fæces is really a secretion from the intestinal follicles, and often from those of *the colon.* I was led to this suspicion by finding 'fresh bile' in fæcal matter which had evidently been lying for a considerable period in the colon, and I propounded the heresy at one of our societies, but received general discredit."

These observations demanded the further inquiry, Is there evidence

that calomel and mercurial compounds act upon the intestines generally, and upon the colon in particular? The answer in the affirmative has been given by Dr. Taylor, who writes, irrespective of the present inquiry, "The coats of the stomach are sometimes corroded; . . similar appearances have been met with in the intestines, *especially in the cæcum.* In a case by Dr. Herapath the stomach was softened, and the cæcum had been the seat of the most violent inflammation, the whole surface being of a deep black red colour, and there were patches of sloughing in the coats" (Taylor *On Poisons*, p. 395). Again (p. 397), "the duodenum and jejunum were healthy; there was slight inflammation of the mucous membrane about the lower two-thirds of the ileum, and this was the more marked towards the termination of the intestine; near to the cæcum there were several patches of inflammation; the whole of the large intestines were highly inflamed; the liver was enlarged and congested; the gall-bladder contracted and containing scarcely any traces of bile." Dr. Scott showed that calomel purges irrespective of bile in the bowels. Coupling these remarks with those of my first communication, the deduction is logical, that there is evidence that mercury *does not increase the hepatic secretion*, but that *it does modify the condition of the intestinal mucous membrane, and especially that of the colon.*—*Brit. Med. Journal, Dec.* 15, 1860, *p.* 985.

27.—CASE OF DISPLACEMENT OF THE LIVER, AND OF OBLITERATION OF THE VENA PORTA.

By A. M. M'Whinnie, Esq.

Whilst attending a young lady with broken rib, occasioned by a fall from her horse, her mother observed that my patient's health, she thought, had suffered from the habit of tight lacing. The menstrual periods were irregular, her aspect was cachectic, and she suffered from hemorrhoids.

On examining into the physical effects produced by the continued constriction of the chest, it was found, by percussion &c., that the liver extended downwards far beyond its proper region; its edge could be felt below the umbilicus, where it seemed pressed down and retained by the constriction of the lower part of the chest.

This case reminded me of some notes and a sketch of the liver of a female, aged twenty-two, who had died of pleuro-pneumonia. Here also the lower ribs were much pressed inwards. On opening the abdomen, the liver appeared to occupy the greater part of the cavity, increased to several times its natural size; and it extended from the hypochondriac region, where the right lobe seemed to press up the diaphragm and encroach upon the chest, downwards to the pelvis; its thickened inferior border reaching to the pubes, dragging down and altering the situation and relations of most of the viscera, the great end of the stomach—as in another instance related to me—being lodged in the left iliac fossa.

The form of the liver from above downwards was altered to a greater extent than I have ever seen in the male subject, and was undoubtedly due to continued external pressure. The ill effects on the functions of many of the organs, &c., must be obvious, and it may be a question how far many of the symptoms in connection with hysteria so eloquently described by Sydenham may not be referable to the above cause.

The return of venous blood we may imagine might be here much impeded, particularly as regards the vena porta. In connexion with this subject, I may cite the following instance of

Obliteration of the Vena Porta.—A woman, aged forty-nine, was admitted into St. Bartholomew's Hospital for dropsy. She stated that she had been in perfect health six weeks previously. The catamenia were at this time suppressed, and the case was treated slightly, with the expectation that on the return of the natural discharges she would recover. She was, however, seized with pain and stercoraceous vomiting, and died. On examination, the vena porta was found obliterated to within two inches of its entrance into the liver. The splenic and superior mesenteric veins were pervious. The gastric veins were turgid, in consequence, no doubt, of their entering the obstructed part of the vena porta. The liver was healthy, and the gall-bladder gorged with bile. Hepatic, cystic, and ductus communis choledochus were pervious. Hardly a vestige of the pancreas, nothing but a very few scirrhus-like lumps, was found, suggesting the idea that the obstruction might have been originally owing to a diseased state of the pancreas. The hepatic artery was of natural size.—*Lancet, Jan.* 6, 1861, *p.* 5.

DISEASES OF THE URINARY ORGANS.

28.—ON THE TWO LEADING TYPICAL FORMS OF BRIGHT'S KIDNEY.

By Dr. S. J. Goodfellow, Physician to, and Lecturer on Medicine at Middlesex Hospital.

[All the various forms of diseased kidney described by writers under the denomination of Bright's disease, may be referred to two leading typical forms: viz, the large white kidney, and the small contracted kidney. These two are essentially different, and never pass one into the other. The fact, however, must be admitted, that the two processes leading to the two different forms of disease, may go on together, and so produce a kidney having a more or less close resemblance to both these forms, either in the same or different parts.]

1. *The Large White Kidney.*—It is, as its name implies, larger than the normal kidney, varying in size from six to twelve ounces or even more; its external cortical, or secreting part is increased at the expense of the internal, medullary, or purely excreting parts, measuring generally between the base of the pyramids and the investing capsule, from half an inch to an inch or more; it is of a whitish or yellowish-white colour, flaccid and anæmic, partly from the obliteration of the vessels, partly from the more or less abundant serosity, and partly from the diminution in the amount of blood-corpuscles. There may, however, be, and there often are, a few enlarged and turgid veins, which in the interior are tortuous, and on the surface have an aborescent form; the medullary portion may present various degrees of engorgement, or may be as exsanguine as the cortical portion, but this, so far as my experience goes, is rare. The course of this disease is generally rapid; it is always accompanied by considerable general dropsy; and the urine is generally scanty, smoky, and contains large quantities of albumen, some blood-casts of tubes, epithelial and exudative casts, and some red blood corpuscles, and, at the later stages of the disease, more or less fatty matter. The duration is seldom longer than from four, five, or six to eight or twelve months; it may, however, persist even longer in some rare cases.

2. *The Small, Contracted Kidney.*—It is invariably smaller than the normal kidney, sometimes only half the natural size; it is hard, contracted, red, and granular; the external or cortical part is wasted, and therefore much diminished, without any corresponding increase of the tubular portion.

This form is chronic in its nature, its duration occupying many years, and is very seldom found without evidence of a similar process in other organs, especially in the liver, and not unfrequently in the spleen and heart; and it is more than probable that these have been affected contemporaneously with the kidney, from the influence of some cause affecting the body generally.

The symptoms of this form are somewhat obscure; certainly not so evident as those characteristic of the other and more acute form. They are more of a secondary character, and more particularly referable to remote organs, rather than to the kidney itself from the effects of the disease, and the retention of the urinary constituents upon the blood, and upon the nutrition of the body. The general tissues of the body suffer degradation, and the subjects of the disease are cut off from some other disease, as apoplexy, or other head affections, pneumonia, pleurisy, pericarditis, peritonitis, &c., towhich they have become predisposed, owing to the general dyscrasia produced by the insidious and unsuspected progress of the kidney affection; they die from pyæmia, erysipelas, or other causes, after accidents or surgical operations which are not in themselves mortal, and not usually followed by a fatal result. The urine in this form is, as a rule, as abundant as in health, sometimes even more abundant; it is of low specific gravity;

it may or may not contain albumen, generally the only effect of applying heat and adding nitric acid is to render the urine slightly opaline, but to produce no sediment or actual precipitate; it scarcely ever contains any casts of tubes, either epithelial or exudative. There is very seldom any anasarca. If there be any, it is small in amount, and then generally only at the close of life. But in many cases, on looking attentively, you may, as I have stated in a preceding lecture, detect a slight œdema beneath the conjunctivæ, and in the feet at night, and a general puffy condition of the eyelids, and of the loose subcutaneous tissues. The more positive or evident symptoms and signs of this form are pallor, neuralgia, headaches resembling those characteristic of hemicrania, noises in the ears, motes before the eyes, and other symptoms referable to the nervous system, dyspeptic and other symptoms indicative of gastric and intestinal irritation, and even inflammation: for even extensive ulceration of the stomach is occasionally found with this condition of kidney, but whether it be an effect of it, experience has not enabled me to decide, although it is not improbable.

Now, as I have said before, and indicated in preceding lectures, every kind of modification of these two forms may be met with, but never any true gradation from the one into the others—that is from the large white, or most acute form, into the hard, contracted, and more chronic disease. There may be, from some peculiarities in the nature of the cause, the habits of life and constitution of the individual, a state partaking of both characters, or the one process may attack a kidney previously the seat of the other process, and so in a manner become engrafted upon it, in which case the organ will of course present the characters of both. It may or may not be larger than normal; generally it is. It is much less pale than the large white; its vessels are much more numerous and more or less gorged with blood; the Malpighian tufts are red and solid, and the organ presents a coarse granular appearance. The symptoms, as you might be prepared to expect, are much less acute than the large white form, and more decided than in the purely chronic form. The urine is very seldom free from albumen, it may contain a very considerable quantity; the specific gravity is considerably under the average, but never so constantly low as in the small contracted kidney; there is almost always more or less deposit of albumino-fibrinous casts of tubes both large and small, and also casts of imperfect broken-down granular epithelium, in which there generally are some minute fatty molecules, and a few isolated blood-corpuscles. There is commonly considerable anasarca, with great proneness to effusions in the serous cavities, and even inflammatory formations—as flakes of lymph, &c.

Besides these two leading typical forms, and the third or mixed form, there are two others mentioned by writers, namely the Waxy, Lardaceous, or Amyloid kidney, and the Fatty kidney. These may be modifications of the first form (the large white), or they may be

produced by an independent morbid process. They much more rarely accompany the hard, contracted type of kidney.

You will see that the terms lardaceous, waxy, and amyloid are synonymous, and are used to indicate the same disease. The first was used originally by Rokitansky, from the supposed resemblance of the kidney when so affected to bacon rind. The second was a name given to it by some English pathologists from a fancied resemblance to wax, and the third is the term given to it by Virchow, because of its offering reactions on the application of iodine and sulphuric acid, resembling, if not identical with, those of the same agents upon starch.

This form of kidney must be rare, for I have not met with it in my practice at this Hospital. But now that we have the means of detecting this degeneration even when partial, and before it has become so general as to affect the whole cortical substance, so as to be apparent to the naked eye, we shall doubtless find it more frequently; for I have for some time had the impression that this change or metamorphosis must be frequent in scrofulous and phthisical patients, and have often been surprised at not discovering palpable evidence of it in the dead-house. Virchow says "that a large proportion of the cases of Bright's disease, especially the chronic ones, are assignable to this change" (into the lardaceous, waxy, or amyloid condition) "and must, therefore, be separated from many other similar forms as constituting a special, altogether a peculiar affection." From the interesting account of this condition given by Dr. Harris, it appears that the kidneys are generally enlarged, and that the cut surface of the cortex is of a pale yellowish-white colour, here and there irregularly depressed, the depressions giving the surface an uneven lobular appearance. The capsules strip off readily, leaving the surface smooth and not torn. The symptoms of this form of kidney are obscure. There is generally anasarca more or less considerable, a great proportion of albumen in the urine, and also some small pale waxy casts, and a few epithelial cells, and red blood-corpuscles. The specific gravity of the urine is generally under 1012. It being a constitutional disease, and the urine containing but a very small proportion of urea, the general symptoms are severe, especially the nervous symptoms.

The next form is the fatty kidney. In most, indeed in all, of the other forms of these affections, the kidney may undergo the fatty metamorphosis, but especially is it prone to take place in the large white kidney, and in the mixed white and granular kidney. Virchow has found that the kidney, whose epithelium has passed into a fatty degeneration, nearly always shrivels up, and the result is a permanent atrophy. But when the pure typical white kidney undergoes this metamorphosis there is but little diminution in size, and the exudative matter deposited in the tubules and interbular substance seems to undergo a still further degeneration into fatty and oily matter. This condition will generally be found in persons who have been

addicted to intemperance—especially in the use of undiluted spirits—as brandy, gin, and whisky.

The symptoms indicative of the fatty kidney will more or less be modified according to the conditions of the organ with which this metamorphosis is associated, and also with the greater or smaller amount of this degeneration in other organs of the body. especially in the heart and arteries. There is generally very considerable anasarca, although great fatty degeneration of the organ has been found where no anasarca was observed during life. The same may be said, also, with regard to albumen. When the white kidney is the seat of the metamorphosis you will invariably find considerable anasarca, and more or less of albumen, and of fat or oily casts in the urine.—*Med. Times and Gazette, Dec.* 1, 1860, *p.* 523.

29.—ON THE EFFECT OF ALCOHOL IN THE INDUCTION OF KIDNEY DISEASE.

By Dr. S. J. Goodfellow, Physician to the Middlesex Hospital.

[Dr. Goodfellow considers it an unquestionable fact, that alcoholic compounds are a very frequent cause of kidney disease and albuminous urine. Moreover these compounds act similarly, whether taken by the stomach or inspired along with the air; hence, disease of the kidney is frequent amongst painters found constantly inhaling the vapour of turpentine, and in tapsters from constantly inhaling the vapour of alcohol, whilst serving at the counter. Dr. Goodfellow proceeds to relate the case of a tapster, in whom nothing had any effect in relieving his albuminous urine until he left off his occupation, and this occurred several times, the albumen returning every time he renewed it. What is the physiological and pathological action of alcoholic compounds?]

Their Physiological Action.—According to current notions, which are founded for the most part upon the theory of Liebig, supported by the experiments and researches of Bouchardat and Sandras, and since also by Duchek, the alcohol passes in the system, principally in the blood, through several oxydising stages, until it ultimately becomes metamorphosed into carbonic acid and water, which are exhaled principally by the lungs. This theory, so plausible, and so satisfactory in many respects, and so calculated to explain many acknowledged effects of alcohol, and supported as it is by the results of direct experiments and chemical analyses made by the first chemists of the day, was generally received as true. According to this theory, alcohol was regarded as an aliment—one of the tertiary, non-azotised aliments,—and therefore subserving the processes of respiration and calorification. The successive changes that alcohol was supposed to undergo in the capillary vessels of the system from the oxygen brought by the blood, were into aldehyde, acetic acid,

and carbonic acid and water. The carbonic acid resulting from the decomposition of the acetic acid was supposed partly and chiefly to get out of the system in a gaseous state by the lungs, partly to become united to several bases, and eliminated by the kidneys and other emunctories. Duchek goes so far as to say that it sometimes becomes converted into oxalic acid. It has been generally observed that the quantity of carbonic acid in the air expired soon after the ingestion of alcohol or any spirituous liquors is very perceptibly diminished, and considerably less than before. To explain this diminution, it has been stated that alcohol, by the action of an oxydising body, loses two equivalents of hydrogen, and gives rise to aldehyde :—

$$C_4\,H_6\,O_2 + O_2 = 2\,H\,O + \underbrace{C_4\,H_4\,O_2}_{\text{Aldehyde.}}$$

At a second degree of oxydation, the alcohol loses two equivalents of hydrogen, which are replaced by two equivalents of oxygen, and acetic acid is produced :—

$$C_4\,H_6\,O_2 + O_4 = 2\,H\,O + \underbrace{C_4\,H_3\,O_3, H\,O}_{\text{Acetic Acid.}}$$

This transformation of alcohol into acetic acid takes place out of the body, as you know, under the influence of ferments, or dry platinum-black. By a further oxydation the acetic acid becomes converted into carbonic acid and water, which are ultimately carried out of the system in the way I have already pointed out.

Nothing apparently could be more satisfactory than this explanation, and you perceive how completely it accounts for the diminution of the quantity of carbonic acid. The alcohol takes all the oxygen, and, therefore, the fatty matters cannot be burnt off; they remain in the system to give rise to the drunkard's fatness, whether deposited as fat, or taking the place of the proper organic principles, and leading to fatty degeneration. The whole of the oxygen is used up in gradually converting the alcohol into carbonic acid, which, as it is slowly evolved, unites with the different alkaline and earthly bases, to be finally eliminated by the kidneys and liver. This was supposed very likely to happen when, from the deadening, numbing, paralysing influence of the alcohol upon the nervous and muscular systems, the respiratory movements became so reduced in frequency and extent, that but little oxygen could be introduced into the blood, from the small quantity of air gaining admission into the lungs.

Now all this theory, plausible and satisfactory as it is, has been completely, and, I think, successfully proved to be false. It will not bear a searching enquiry into the true facts of the case. And after all, as the sequel will prove, we are obliged to come back to the old opinions, as derived from unbiassed experiments, and before facts

were made to square with chemical theories. MM. Lallemand. Perrin, and Duroy have made this inquiry in the true spirit of philosophy, and in the most searching manner. It is impossible to read the account of their experiments and analyses without being convinced that they had one object alone in view, and that was truth, apart from any preconceived views or theories. It is altogether out of the scope of these lectures to quote at length the beautiful, ingenious, and very satisfactory experiments by which they have been led irresistibly to their conclusions. Suffice it to say that the results of their experiments admitted of no other conclusions than those which the authors came to, and which are of great value in explaining the pathogenic action of alcoholic and allied substances. These gentlemen then have found upon evidence, which I do not see how any one can gainsay, that whether alcohol, or its compounds brandy, rum, gin, or whisky, be taken into the stomach, or inhaled by the lungs, it is only found as alcohol in the blood and in the tissues, especially in the nervous substance, for which it would seem to have a special attraction; and that it has no claim to be regarded as an aliment. When taken into the stomach some small portion may become converted into acetic acid, by the gastric juice and the mucus acting as ferments. But even this small quantity does not enter the blood. In this list fermented drinks which contain more or less nutrient matter mixed with the alcohol must be excluded, such as wine, beer, cider, perry, &c. Wines contain even nitrogenous matters, also colouring and fatty matters, and salts; cider contains glucose, mucilage, vegetable acids, &c.; beer also contains glucose, dextrine, and allied substances in considerable proportions, nitrogenous matters, bitter and aromatic principles, and salts. According, then, to MM. Lallemand, Perrin, and Duroy, alcohol is neither transformed nor destroyed in the organism, and is ultimately eliminated without undergoing any modification. They have detected it in considerable quantity in the blood, brain-substance, when freed from membranes and blood, and in the urine by means of distillation; they have shown afterwards by the aid of exact doses analogous to the process of analysis by the method of volumes, that alcohol diffuses itself in the tissues, and that it accumulates in the brain, and in the liver, where it is found in larger quantities than in the blood and other organs. They have proved by multiplied experiments, verified by counter-proofs, that alcohol does not undergo any modification in the economy, and that it does not give rise consequently, to any bodies resulting from its oxydation, such as aldehyde, acetic acid, &c. It is only in the stomach that it is susceptible of experiencing any modification, for a small fraction of alcohol ingested is there converted into acetic acid by the action of the gastric juice and the mucus, which act then as a ferment; but this action, altogether local and special to the stomach, ceases the moment the alcohol penetrates the venous radicles. These authors have shown, moreover, that it is

eliminated by the lungs, the skin, and the kidneys, as alcohol. It is not only after the ingestion of a *great* quantity of alcohol that they met with it in the organs, for they found it in the blood of a dog, nine hours after he had taken only 30 grammes (3 drachms 37½ grains) at 21°; they met with it in a man who had drunk about 30 grammes (about 3½ drachms) of brandy; they observed, finally, that the pulmonary exhalation of a man who had taken a litre (7-8ths of an Imperial quart) of wine, of a middling alcoholic richness, contained alcohol for eight hours after taking it, and that the urinary secretion gave evidence of its presence during fourteen hours. The authors may well ask,—"Is this the mode of action of an aliment?" All the tests for the detection of aldehyde, and acetic acid, were had recourse to after alcohol had been administered in various ways, and in every dose, but without avail; whereas when very small quantities of these substances were administered, evident indications of their presence in the blood, and in the organs, and in the exhalations from the lungs, were at once observed. I have already described how the diminished quantity of carbonic acid exhaled after the ingestion of alcohol, was explained under the old theory. It remains to show how the diminution can be accounted for under these observed, indisputable facts. Now, it appears, from the researches of MM. Lallemand, Perrin, and Duroy, that alcoholic substances exert a very remarkable action upon the blood, which presents in animals alcoholised numerous globules of fat, like cholesterine, visible to the naked eye, and swimming on the surface of that fluid. This is of such interest in connexion with our subject, that I shall again refer to it when I come to speak of the pathological effects of alcohol. At present it is in its physiological aspect that they are of importance. Since alcohol produces a modification so singular, may it not offer also, when present, an obstacle to the disengagement of carbonic acid, or delay even the combination of oxygen with the carbon of the blood? If this be the case, alcohol contributes to nutrition, not actively as an alimentary substance, but in an indirect manner in exercising a moderating influence upon organic decomposition. With respect to its influence independently of this separation of the fatty principles, and perhaps their conversion into a non-saponifiable state, but little is positively known. It is conceivable, however, from the properties which alcohol is known to possess,—its great diffusiveness through, and attraction for, water,—its power of dissolving some very important animal principles, and of coagulating others,—that it does exert a considerable influence upon the physical, and also probably upon the chemical qualities of the blood, and blood-corpuscles. Nothing definite, however, has been observed. Dr. Addison, of Brighton, whose able researches have thrown light upon some physiological and pathological processes, has observed some very curious effects upon adding sherry wine to blood out of the body; and it is possible that alcohol, when taken into the system in large

quantities, may in time work such changes, and even destroy the red corpuscles already formed, and hinder the full development of others. MM. Lallemand, Perrin, and Duroy, however, saw no alteration in the corpuscles, even when alcohol was added to the blood out of the body, and also in blood taken after large quantities had been imbibed.

Poiseuille's experiments proved that its mixture with the animal fluids both when directly injected into the blood-vessels, and after being taken into the stomach, retards the circulation through the capillaries, although its first effect is to excite the heart to increased action. It diminishes the want of food, and impairs or destroys the appetite for it. Bouchardat remarks that with drinkers of brandy and other alcoholic liquors, the alcohol acts by diminishing and suppressing probably the functions of absorption by the stomach in respect of every other substance; it augments, on the contrary, the secretion of that organ; and from these conditions arise the increased secretion of mucus, the disgust for food, and the emaciation. Of course such liquids as beer, some wines, and cider, and other nutritious and true alimentary and fattening drinks, are not included.

That it affects the nervous system, and indirectly, if not directly, the muscular system also, I need scarcely mention; it is too often rendered obvious to us. A moderate quantity produces an excitation of the nervous system, which extends over the whole economy; a still larger dose produces great disturbance of the cerebral functions, which another and still larger dose completely annihilates. The same effects nearly are observed upon the muscular system. A moderate dose seems to impart strength to the muscular contractions, while a very large dose destroys all voluntary contractility, and a poisonous one that also of the involuntary muscles. Flourens' experiments upon the effects of alcohol upon birds are very instructive. Its effect upon them resembled that produced by the removal of the cerebellum, except that the intelligence remained. With alcohol, I need not say it was destroyed. In poisoning by alcohol the respiratory movements and those of the heart were the last affected,—those of the heart the last. Even for some considerable time after respiration had ceased, the heart continued to beat. In the experiments which I made upon the frog, which some of you witnessed, the heart continued to beat, the circulation went on, for some time, after respiration had ceased.

Their Pathological Effects.—That alcohol is a local irritant is unquestionable, and that it produces its effects upon the system partly in this way is very probable. It may act remotely by sympathy to some small extent, as Orfila believed. But we have seen from the very able researches of MM. Lallemand, Perrin, and Duroy, from whose book I have already quoted so largely, that it is rapidly absorbed by the venous radicles, and that its principal action is directly upon the different organs which it irritates, and eventually inflames. Especially has it been proved to be present in greater proportion in the nervous tissue than elsewhere, which it more particularly excites.

It disturbs its functions; it perverts and ultimately destroys the intellectual faculties, and even the emotional faculties; it disturbs the function of the sensory nerves, both common and special, as shown by subjective tactile phenomena, strange perversions of taste, double vision, and other disorders of the optic nerves, tinnitus aurium, and other disorders of the auditory nerves. It equally disorders and destroys the function of the motor nerves, as shown in irregularity, and absence of consentaneous action of the movements. From these effects upon the cerebro-spinal system it is more than probable, that it disturbs and impairs the functions of the organic nervous system, as evidenced by defective nutrition and secretion. When taken in the form of brandy, whisky, gin, and such fluids, it impairs nutrition, probably from its great attraction for water, inspissating the blood and juices of the body. I need not mention in what large proportion water enters into the composition of the tissues and fluids of the body. It is probably in this way that it acts as a diuretic so far as the increase of the watery part of the urine is concerned, not only from the increased quantity of water ingested with and after the brandy, but from its abstracting it from the tissues. There is no doubt that it tends to harden the brain substance, and produce atrophy of many of the structures, not only by increasing the quantity of connective tissue and other white fibrous tissues, and so leading to undue pressure upon the more important parts, but by condensing the tissues directly by the abstraction of water. There is no doubt of its exerting this destroying influence upon the liver. I shall endeavour to show you that it does so upon the kidney also. As a general rule, it irritates and inflames the tissues of the stomach and duodenum, and even the pancreatic and hepatic ducts, and it probably affects and deteriorates the secretion of these glands. It produces hypertrophy of the connective tissue forming Glisson's capsule, which in its turn, presses upon the small vessels, and upon the hepatic cells, and produces atrophy of these anatomical elements in two ways; first, by cutting off the supply of nutrient materials, and secondly, by absorption from pressure. The digestive processes are probably still more impaired by the bad quality of the bile and pancreatic secretion.

Now, very much the same changes take place in the kidney as in the liver, and other organs. We have seen that alcohol passes through the vessels and tissues of this organ as alcohol; it irritates these tissues, as it does similar tissues in other parts; it leads to blood delay; it impairs the influence and function of the nervous system; it produces hypertrophy of the connective tissue, forming the stroma or framework of the organ, and of the capsule; and it produces a granular appearance precisely as it does in the liver. In fact, this alteration is very commonly seen in both these organs in old drunkards, especially and almost exclusively those who take the raw spirit in large quantities, or spirit mixed with only small quantities of water. Those who drink largely of beer and perhaps of wine, are found to have a

somewhat different form of kidney, especially when taken as gin, brandy, &c. But we have seen that alcohol separates and modifies the fatty matters of the blood. MM. Lallemand, Perrin, and Duroy, have seen this. Most pathologists believed that so far as the relation between cause and effect could be traced, it was almost certain that alcoholic beverages, when largely and continuously consumed for any length of time, led to fatty degradation. This separation has now been actually seen and proved. Now, this separation and alteration of the fatty principles of the blood probably plays a very important part in the pathological effect of alcohol, when taken in large quantities, in the form of brandy, gin, whisky, &c. Now, saponifiable fatty matters, that are visible to the naked eye, are calculated to impede the circulation through the capillaries—if not to cut off the blood-supply altogether,—and so produce atrophy of the secreting tissues, while the connective tissue, supporting the vessels, would receive an undue supply of blood plasma, and therefore become hypertrophied. It is not improbable that some of these fatty matters become transuded with the exudates, and thus lead to the presence of fat in the tubules, and also in the intertubular substance; some may also remain in the walls of the capillary vessels, and replace in time the normal elements.—*Med. Times and Gazette, Nov.* 17, 1860, *p.* 471.

30.—ON THE TREATMENT OF BRIGHT'S DISEASE.

By Dr. S. J. GOODFELLOW, Physician to the Middlesex Hospital.

[One very important, but difficult and delicate part of our treatment, consists in regulating, or removing the morbid action of distant organs produced by the supplementary action imposed on them, by the imperfect secreting power of the kidneys. Sometimes the perspirations are profuse: this is favourable; for the skin is the only safe emunctory for the elimination of the urinary constituents. When the vicarious action is in the gastric intestinal mucous membrane, or in any serous membrane, we must try to divert it to the skin. These effusions are perhaps more properly called secretions, as the urinary constituents are really eliminated by them.]

These effusions in Bright's disease afford great temporary relief to the more important symptoms. They are in fact a physical necessity, in consequence of the great distension of the vessels, and the almost stagnant condition of the blood in them. Absorption is all but arrested under these circumstances. To apply, therefore, a roller to the lower extremities, will have for its effect only to remove the fluid to parts, the function of which is much more important, and even immediately essential to life. Great caution, therefore, I repeat, should be observed in the application of this bandage in all cases of dropsy, but especially in dropsy depending upon these diseases of the kidney. As a general rule, it is never justifiable, so long as the ana-

sarca seems to be on the increase. Under such circumstances, it is far better, and less dangerous, that vesication should take place, or even acupuncture be resorted to, and the serum allowed gradually to ooze away. It is singular, but nevertheless true, though not easy of explanation, that spontaneous vesications, indeed actual sloughing sores, on the lower extremities, are much less liable to spread than acupunctures, or similar small punctures made with the lancet. It is not an uncommon occurrence to see patients brought into the hospital with enormous unhealthy-looking vesications, and even sloughing sores of various sizes, from which there is a constant and copious discharge of serosity, and they generally do well; while punctures, however minute, made with the lancet, or with the grooved needle, are very often followed by rapidly-spreading erysipelas, notwithstanding that every precaution is taken to keep up the temperature of the limb, and to absorb the serosity as it flows out, by enveloping the limbs in cotton-wool and flannel.

[The author now proceeds to the treatment of each form separately.]

First, with reference to the Acute Forms when apart of Scarlet Fever.—As prevention is, by common consent, better than a cure, a few words on the treatment of scarlet fever, more especially with reference to the prevention of kidney-disease, will not be without their value. Besides, such a mode of treating the subject will be in strict accordance with the general plan upon which I have proceeded all along, and that is, to consider these affections of the kidney like other morbid affections of the system. There is nothing, in fact, specific in these diseases. They are referable, as I have endeavoured to show, to the same causation, and it is only an accidental circumstance which determines the operation of the particular cause to the kidney; or, as with many of the causes indeed, the kidney-disease is only a part of a general disease, affecting more or less, and with greater or less intensity, according to circumstances, other organs of the body. The treatment must be guided, moreover, by the same general principles. In a mild form of scarlatina but little treatment is necessary. To secure a moderate but efficient action of the bowels, and of the skin and kidneys, and protect the patient against exposure to cold is all that is necessary. The action of the bowels is best secured by a dose of castor-oil, or the saline draught, that of the skin and kidneys by the haustus ammoniæ acetatis. As long as the disease has every appearance of remaining mild, I do not, as a rule, use any other treatment. If, however, there is the slightest indication of its assuming a more severe, or a malignant form, I immediately prescribe iron and quinine, or iron alone, and always in the form of the tincture of the sesquichloride. In all cases of scarlatina (and the same remark is applicable to all diseases which are the result of the action of some animal poison), you should not discontinue your observation of the case until you have the strongest grounds for believing that all the

poison has been eliminated from the system. and that the action to which its development and increase were due, has entirely ceased. This is not always—indeed, scarcely ever—the case in that state which we describe by the term Convalescence. In all cases, however mild, this is not accomplished for some weeks after this.

During convalescence, with the view of getting rid of the poison altogether, and during the active stages of the disease to favour its elimination from the body, and prevent its further development and increase in it, it is of great consequence that the atmosphere should be changed often. During convalescence, if the weather be mild, the patient should go into the open air, at the same time being warmly clad. If in winter. the air of the house or chamber should be entirely changed once or twice a-day. If he be still in bed, and the fever is of a severe or malignant form, this change becomes essential for the patient's safety. Now, the atmosphere of a room cannot be freed from impurities while the walls remain standing. Don't be startled. New walls, for my purpose, can be built with lime. All animal poisons certainly, all atmospheric impurities probably, have a great attraction —an *adhesive* attraction for surfaces. The rougher they are the greater the attraction. The following fact, among many others of a similar although less striking character, will show this:—When I was a resident at the London Fever Hospital, a patient was brought to the Hospital suffering from erysipelas of the face and scalp, which had come on during an attack of fever. In this ward (which was in the form of an oblong parallelogram) were thirteen beds, six on one side, and seven on the other, the space that should have been occupied by the seventh bed being taken up by door-ways leading to, and partitions separating, the latrines, and sculleries, and bath-rooms. The patient was placed in the bed next but one to the end of the ward opposite to the scullery, and on the side on which there were seven beds. The patient in the end bed, on the same side, was attacked, then the patient in the next bed (the third from the end), and then in succession in every bed, until it had attacked the one in the last bed at the bottom of the ward. It then attacked the patient in the last bed on the opposite side, and so in succession up to the fifth bed, and as far as the offices above mentioned. The only patient who escaped was in the bed *beyond* the offices. From this circumstance, and others like it, I determined, on my own responsibility, to have the walls lime-whited every month or so, according to the number of patients constantly in the ward. The records of the hospital will show that previously, erysipelas, pyæmia, and a peculiar form of laryngitis attacked great numbers, and were very fatal; but afterwards no case occurred, up to the time of my leaving the hospital, which was some two or three years, notwithstanding that during that time several cases of erysipelas were admitted.

There is one other point connected with the treatment, which I feel bound to refer to, because I think that it is influential as a pre-

ventive of these diseases, as well as eminently curative of the scarlet fever itself. I allude to the practice of giving tonics, such as quinine and iron, or iron alone, in tolerably large doses, proportioned to the severity or malignancy of the disease. I have been in the habit for many years past of prescribing these tonics in bad forms of scarlatina, securing, at the same time, a moderate but still efficient relief to the bowels once or twice daily by castor-oil or some other mild aperient, with the most favourable results; nor (which is of more interest in connexion with our subject) has any patient become affected with dropsy, so far as I have been able to ascertain—certainly not in consulting practice. In hospital practice one cannot be so certain, for it is possible that some may have been attacked after they have been dismissed; but as most of the patients come from the neighbourhood of the hospital, it is most probable that they would have returned if they had been attacked with dropsy. But suppose, in spite of all your precautions, a patient is attacked with this acute form of diseased kidney, accompanied by inflammatory anasarca, what steps are we to take, first, to prevent a partial or complete suppression of urine, and our patient from being carried off by convulsions and other modes of death under these conditions; and, secondly, to restore the organ to a perfectly healthy state, and so avoid its merging into the chronic, or large white kidney? This treatment must be modified according to whether the disease follows a malignant form of scarlatina, or is a sequel to a mild form of the fever. If in the latter case the patient will bear and will require a far more active treatment than in the former, and as a general rule, those of you who may settle in the country will find it necessary to treat it more actively by depletion, than those whose future labours may be in London or other large towns. I can most confidently inculcate this from a somewhat large experience in both. In the acute form, then, it may be necessary to take blood from the arm, the quantity being regulated by the urgency of the symptoms. Whenever the patient is threatened by convulsions soon after the commencement of the attack, this should never be omitted. I know no exception to this, except in the case of one who is anæmic, or who has been much weakened previously by the scarlet fever. In this case we must be satisfied with abstracting blood from the loins by leeches or cupping. This local abstraction of blood is of great service. Generally speaking, patients labouring under this form of kidney disease bear bleeding well. Next in efficacy, as in order of action, is the warm bath, great care being taken to prevent a chill after the removal from the bath. To avoid all risk of this the patient should be well rubbed by dry and warm towels, immediately that he rises out of the water, then placed in warm blankets, and put to bed. The warm bath is far more efficacious than the vapour or air bath, and the patient should remain in it until he makes complaint of a feeling af fatigue or of faintness. In country practice, and with the poorer class of persons, except in the case of children, this important remedy

is not attainable. Under these circumstances a very efficient substitute can be extemporised, if the symptoms be such as to lead the practitioner to think that this mode of equalising the circulation, and of securing a free transpiration through the skin is called for,—let the patient's bed be brought near a fire, or if he be strong enough, let him sit on a chair, and well supported. In either case he should be closely enveloped in flannel—for example, a blanket, and over this, two or three other blankets are to be loosely thrown, so that a sort of chamber may be formed between them. Make a tube of about one inch or one and a-half inch bore, by rolling two or three newspapers, or other large sheets of paper; introduce one end into the chamber so constructed, and place the other end over the spout of a kettle half filled with water in an active state of ebullition, and a most efficient vapour-bath is thus formed. The only precaution to be observed with respect to this extemporaneous vapour-bath is, not to place the end of the tube too near the patient's person, or too long in one situation but to let the jet of steam fall in different parts. An early administration of an efficient aperient is always called for, even if the bowels have been moved spontaneously before your visit. The only circumstance prohibiting the administration of a purgative is actual looseness of the bowels—or rather actual diarrhœa. In some instances mere looseness may be co-incident with a loaded state of the bowels; if, therefore, you find the belly tumid and not very resonant, you will do well even in the case of there being a slight looseness, to give a dose of the compound jalap-powder; for example, from a scruple to half a drachm or even two scruples according to the strength of the patient; if with children, the dose must of course be smaller. It is better that you should be called upon to administer a warm stimulant for the weakness, and to support the strength, than that a free action of the bowels, and of the skin, should not be produced, and due depletion resorted to. I could relate many cases where patients—chiefly young persons—have been seized with convulsions a few hours after being attacked with this form of kidney disease and anasarca, and have been placed out of danger in a few hours by a full abstraction of blood from the arm, followed by leeches at the loins and the warm-bath. If the compound jalap-powder be not hand, or there be a disposition to vomiting, I see no objection, as some appear to do, to the saline aperients. If there be any nausea or vomiting, a dose of Epsom salts, with plenty of water, not only quickly produces several loose and even watery evacuations from the bowels, but also allays the disposition to sickness. These, then, are the great and immediate remedies that will be called for in most cases. The extent to which they should be carried, or whether one or more need be resorted to, will depend on the urgency of the case. This must be left to your own judgment. Bear in mind, as a golden rule, in the treatment of all diseases that it is better to err on the side of too little activity in the treatment than on that of too much. Now, in most cases, you will

have to continue your treatment. You may not have had an opportunity of attacking the disease with these remedies at a sufficiently early period, or at the onset of the attack. Well, then, it is necessary to keep up the action of the bowels daily, either by the compound jalap-powder, or the saline draughts. Two or three loose motions at least should be secured daily. You have seen in my practice at the hospital that in almost every case I order the haustus magnesiæ compositus of the Hospital Pharmacopœia, every four, six, or eight hours, according to the effect, (magnes. carb. gr. v.; magnes. sulph. ℥j.; aquæ menth. pip. ℥xij. Misce), with the most favourable results. You can bear witness that we have not lost a case in this acute form of the disease. Some recommend, and I have tried, the antimonial treatment,—either ten, fifteen, or twenty minims of the vinum antimonii potassio-tartratis in the haustus ammoniæ acetatis, every three, four, or six hours; or antim. pot.-tart. gr. $\frac{1}{8}$ to $\frac{1}{4}$ in a pill, with ext. hyos. grs. iij. to iv. From an experience of both of these medicines, I must confess I prefer the former, more especially as the patients are generally troubled with nausea, and, frequently, with vomiting. When this symptom is urgent, recollect that it is most probably (in the early stage of the acute affection) due to sympathy with the kidneys; and is an index, one may say, of the severity of the process going on there. In many cases it will be better under these circumstances to allow the stomach to remain at perfect rest, and not to administer any internal remedies. You will find the application of a few leeches to the loins, followed by the warm bath, and, if the bowels are confined, a laxative enema, a much more efficient means of arresting the vomiting than the internal administration of any medicines, the presence of which the irritable stomach will not tolerate, and a persistence in which is, therefore, calculated rather to keep up this embarrassing and exhausting symptom than to allay it. This symptom is often associated with severe headache, and tendency to convulsions, especially in children and young persons, and the same treatment will be equally appropriate for both. The constant application of a large linseed poultice, with which a few grains (a pinch or so) of mustard may be mixed, or of a large Markwick's epithem, or a piece of flannel, previously dipped in hot water and well wrung, will be of great service. If the flannel is used, it should be covered with gutta-percha, and over that by a thick layer of cotton-wool. If the haustus magnesiæ comp. after a few days produces great flatulency, and intestinal distension, which it sometimes does, you may then give the haustus ammoniæ acetatis, with one, two, or three minims of the dilute hydrocyanic acid, and a drachm, or two drachms, of the infusion of digitalis. In children the dose must be regulated accordingly, or these latter omitted. The last will be a safe diuretic at this period of the disease, and the hydrocyanic acid will tend to quiet the irritability of the stomach. With respect to diuretics in general especially the stimulating diuretics, they are altogether

contra-indicated: they are calculated in these acute forms to do a great deal of mischief. This does not apply, however, to the haustus ammoniæ acetatis; its acts as a diuretic, as I conclude from experience, and from the results of Poiseuille's experiments, by the rapidity with which it passes through vessels, not seemingly by stimulating their walls, but by some other property. As a general rule elaterium, and other drastic purgatives, are not advisable in these acute affections. It is true that during their action they produce several loose and watery evacuations, and may afford temporary relief to the system, yet they unduly stimulate the mucous membrane, and render it, or its capillaries, unfit for the continuance of profuse secretion. The Epsom, or what is better, although undeservedly but little used, the Glauber's salts, act even more efficiently in the first place, and do not leave the membrane and other capillaries (so to say) exhausted.

At first, the severest antiphlogistic diet should alone be permitted. In general, but little diet is wanted. The patient ought to be restricted to gruel, arrowroot, and such articles of food, and no nitrogenous matters allowed,—such as beef-tea, animal broths, and so on. It is impossible, however, to decide for every case. You must exercise your own judgment, and be guided in this respect by the symptoms and state of your patient. After the acute stage is over, and the urine has become abundant, the tube casts—epithelial and bloody,—and the blood-corpuscles have disappeared, the skin has become cool and soft, and the pulse reduced to nearly the natural standard, and the other feverish symptoms subsided: if there be still anasarca, and there be very evident anæmia, you may prescribe the tincture of the sesquichloride of iron in doses of five, ten, or fifteen minims (according to the age and condition of your patient), three or four times daily, in a small wineglassful of water, and you may cautiously increase these doses. I seldom, however, find it necessary in these affections and under these circumstances, to give more than fifteen minims even to an adult. This preparation (most probably by its styptic properties) will, generally, lead to the disappearance of the albumen, at the same time that it improves the quality of the blood, and gives tone to the system generally.

When the disease is produced by cold, the same general principles of treatment are to be acted upon. The only difference is, that the patients, when affected from this cause, bear depletion better than after scarlatina, and the warm baths should be more frequently repeated.

When it is the result of alcoholic poisoning, or any other direct irritant of the tissues of the organ, the cause acting more locally, general depletion will rarely, if ever, be called for. But local depletion ought to be more freely resorted to, and the action of the bowels promoted by enemata, in preference to purgatives by the mouth.

This, then, is the plan of treatment which I have found most successful in the acute forms of these diseases. In all cases you must be guided by the symptoms. In scarcely any will stimulation be justifiable.—*Med. Times and Gazette, Dec.* 22, 1860, *p.* 601.

31.—ON THE ORIGIN OF UREA.

By Dr. GEORGE E. DAY, Professor of Medicine in the University of St. Andrews.

[In the following extract from his recent work on 'Chemistry in its relations to Physiology and Medicine,' Dr. Day has succeeded admirably in throwing together, within a reasonable compass, all that is known on the origin of urea.]

It is well known that the origin of urea is still a *quæstio vexata* amongst chemists and physiologists; one party, including the names of Liebig, Bischoff, and others, holding that the urea is solely a product of the metamorphosis of the nitrogenous tissues; whilst the other party, which ranks amongst its supporters Lehmann, Frerichs, and (more especially) Bidder and Schmidt, maintain that the formation of urea is dependent upon two factors, one of which is variable, namely, the amount of assimilated histoplastic or albuminous food; whilst the other is constant, namely, the necessary consumption of the albuminous tissue when the animal is fasting.

It admits of no doubt that urea is formed from the nitrogenous constituents of the organism, its artificial production from such substances affording the strongest evidence on that point; in addition to which we may add the facts observed by Lassaigne, Scherer, and others, of urea being contained in the urine excreted after nearly three weeks' starvation. As the metamorphosis of tissue occurs with the greatest activity in the muscular system, and as, further, increased bodily exercise augments the amount of urea, we are justified in regarding the urea as formed for the most part from the worn-out muscular fibres, although it is most probable that other vital tissues may contribute to the general amount. Whether it is formed in the organic particles at the moment of their disintegration, or whether it is first formed in the blood, is a point which cannot be considered as decisively established; but it is most probable that the latter is the correct view, because Liebig, in his experiments on large quantities of muscular juice, could not detect in it any trace of urea, although he found substances from which he could produce it artificially. It seems, therefore, almost certain that these substances (creatine, creatinine, and probably inosic acid) are decomposed in the blood, by the action of the alkalies, and of free oxygen, into urea and other matters to be excreted. Moreover, the view that the urea is formed in the blood is supported by well-known experiments, showing that gelatin, glycine, alloxantin, theine, and other substances, which it is impossible to suppose can form tissue, are converted into urea and other matters, as is evidenced by the fact that this substance occurs in a perceptibly increased quantity in the urine, soon after any of the above-named substances have been swallowed.

Lehmann's view that the urea is in part formed from assimilated nitrogenous food which has never entered into the substances of the

tissues, is chiefly based on the following facts:—(1.) on the extent to which its amount is increased by the free use of animal food (nature in this way getting rid of the superfluous plastic material along with that which has become unfit for use); and (2.) on the circumstance first noticed by Frerichs, but mainly established by the investigations of his opponent, Bischoff, that the use of gelatin and gelatinous food so rapidly increases the quantity of urea, that we are compelled to believe that these nitrogenous matters are at once directly oxidised in the blood, without having entered into the composition of the tissues; and if these, why not the protein bodies also? (We should observe that Bischoff himself fully grants that the gelatin is directly converted in the blood into urea, but, he adds, it is never a natural article of food, nor is it ever found as a normal constituent of the blood).

When treating of uric acid we shall show that in all probability a great part of the urea, separated by the kidneys from the blood, had previously existed in the form of that organic acid.

Whether urea exerts any special influence on the fluids of the eye is a question that no one has yet attempted to answer.—*Glasgow Med. Journal, Jan.* 1861, *p.* 464.

32.—ON THE INFLUENCE EXERCISED BY VARIOUS DISEASES ON THE QUANTITY OF UREA EXCRETED.

By Dr. GEORGE E. DAY, Professor of Medicine at the University of St. Andrews.

To the remarks which have been previously made on the influence of disease in modifying the amount of urea we may add the following. In intermittent fevers it has been found (by Moos, Traube, and Redtenbacher) that, during the paroxysm, the secretion of urea and of urine generally is absolutely increased, while in the period of intermission there is a diminution both of the urea and of the urine generally. In the cold and hot stages about 3·5 times, and in the sweating stage about 0·5, more urea are secreted than in the period of intermission.

Warncke's investigations in reference to typhoid fever are of such importance that we shall borrow freely from his memoir. "The urine of a patient labouring under typhoid fever is not distinguishable by external characters from any other. It may be pale, may be excreted in large quantity, and may be clear or may be turbid, opaque, deposit a copious sediment, and be secreted in small quantity (which is particularly the case in the commencement of the disease); but its special characteristic is that it contains *an absolute and relative increase of* urea.

"This property is retained by the urine through the stages to which the disease may be said to increase; when, on the contrary, the latter diminishes, the amount of urea is likewise lessened, and the quantity

continues smaller after the end of convalescence, until the restitution of the body is completely effected.

"The following table exhibits the average numbers of more than fifty investigations (on thirty men and twenty women), which were repeated daily during the stay of the individuals in the hospital:—

	Males.	Females.
In the first week	43·2 grammes	..... 34·0 grammes
" second "	39·9 "	..... 30·2 "
" third "	30·9 "	..... 24·1 "
" fourth "	23·2 "	..... 20·5 "

"If these numbers be compared with those representing the quantity of urea which is normally excreted, especially under the use of vegetable food, and there are very few patients that use even it, it must be allowed that increase of the quantity of urea is a constant phenomenon in typhoid fever. This character, however, is not peculiar to typhoid fever, for in many other acute diseases, as in pneumonia, pleurisy, rheumatic fever, &c., the quantity of urea is increased; but in all these the other organic constituents of the urine are simultaneously augmented, while this takes place with the urea in a less degree. We cannot attach too much importance to this circumstance. An increased quantity of organic matters in general indicates an augmented supply of combustible materials; but an increased quantity of urea without any simultaneous augmentation of the other organic compounds in the urine, as is the case in typhoid fever, points, on the contrary, to a more energetic combustion." Warncke goes on to show that the increase of the quantity of urea is not equally great in all typhoid fevers, and that it varies directly with the height of the temperature, the rapidity of the pulse, and the degree of emaciation.

A diminution of the quantity of the urea, though never to a degree below the normal standard, occasionally (but only seldom) took place; it was especially observed after violent hemorrhage from the bowels, and when there was considerable enlargement of the spleen.

In many cases the patients, during the entire course of the disease, took food regularly; but in none of them was the consumption of food followed by an increased quantity of urea.

Warncke lays great stress on the value of the determination of the urea in reference to differential diagnosis. "When I find," he observes, "the increase of urea without any augmentation of the other organic constituents of the urine to be peculiar to typhoid fever, I am easily led to seek in this fact a diagnostic sign between this affection and the diseases which in other symptoms resemble it, especially gastric fever and meningitis."

In gastric fever he found that the quantity of urea was not very different from what is excreted by healthy persons when living on a vegetable diet. From an extended series of analyses he obtained the following mean numbers:—

	Males.	Females.
For the first week	..... 22·1 grammes	..... 18·0 grammes
" second "	..... 24·2 "	..... 19·8 "
" third "	..... 25·7 "	..... 20·4 "

Thus we have here the reverse of what occurs in typhoid fever. The quantity of urea is less in the first weeks than during convalescence.

In meningitis the differences in relation to the amount of urea are at least equally marked, according to Warncke. In a boy aged seven years, who died from meningitis, the mean daily quantity of urea was 6·4 grammes, while in a boy aged eight years, who died from typhoid fever, the minimum quantity of urea was 11·9 grammes. Heller mentions meningitis as a disease characterised by an excessive excretion of urea. If Warncke's views are correct, it is possible that in Heller's cases there was an error in diagnosis.

In diabetes there seems (from the investigations of Thierfelder and Uhle) to be a definite ratio between the daily quantity of nitrogenous food and the daily excretion of urea."—*Glasgow Med. Journal, Jan.* 1861, *p.* 467.

33.—ON URÆMIA.

By Dr. RICHARDSON.

[The following is an abstract of a paper read before the Medical Society of London. The author first pointed out the analogies between uræmic poisoning and poisoning by certain common narcotic substances, and then passed on to the symptoms and diagnosis from other forms of disease. Three points in this part of the paper may be mentioned as of interest.]

The pupil, Dr. Richardson has observed, is usually fixed in uræmia, and, in most cases, is dilated; but, he added, this rule is not without exception, for he has seen the pupil contracted to a pin's point in a case of unmistakeable uræmia. There is in some cases, as Frerichs has said, evidence of an excess of ammonia in the breath during the acute attack; but this is not universal, and hence some have denied it altogether. The reason of a difference in this respect in different cases, is very simple. In persons suffering from kidney disease, and in whom uræmia is a probable occurrence, the breath at the best of times is charged with ammonia to an extent greater than is normal. In these cases the lung is supplementing the kidney, and the elimination of the ammoniacal product is, in fact, the saving clause. It is when such persons take congestion of the lung, and are subjected to diminution of excretion by the lung, that the uræmic symptoms advance; and in these cases the breath is not ammoniacal during the attack. But there are other examples, where the uræmia is sudden in its appearance, owing to sudden arrest in the function of the

kidney simply. Then the breath is markedly ammoniacal in the period of the acute attack. The third fact, as diagnostic of uræmic poisoning from poisoning by the ordinary narcotics, is, that during uræmic coma the patient will often rally and regain all his consciousness for a time, sinking again into forgetfulness, and even dying unconscious in the end.

The cause of death in uræmia formed the matter of another section of the paper, and was followed by observations on treatment. In the treatment of uræmic narcotism coming on suddenly, in a person not debilitated by previous disease, and not overloaded with fat, Dr. Richardson stated his belief that there was one ready and direct remedy, and that was free bloodletting. He had seen a man who had lain three days comatose and unconscious recover, under the immediate influence of loss of blood, so completely as to transact business affairs, and inquire into all that had occurred since he was struck down. Moreover physiological reasons supported this treatment; for the bloodletting not only relieved the body from a portion of the poison, but removed the congestion of the kidney and of the other organs, and gave the permit for recovery, if recovery were possible. Thus, in animals in which artificial uræmia had been produced, the effect of frequent venesection tended greatly to prolong life. That bloodletting should not absolutely relieve in every case was reasonable, for whether relief were obtainable or not in any case would depend upon the degree of mechanical obstruction in the kidney; for if the obstruction were perfect, no treatment would be possible, seeing that no proceeding could be adopted to supplement the kidney altogether; but if in any instance there should be but partial obstruction, increased temporarily by congestion, then the act of abstraction of blood gave the only chance that remained of removing the burthen from the excreting organ. The last part of the paper dwelt on uræmia in its forensic aspects. In many cases, where death is supposed to have occurred from the effects of small doses of opium or other narcotic, he (Dr. Richardson) believed that the cause was attributable to uræmia, and that so-called idiosyncrasies were probably intimately connected with renal disorder.

He did not believe the poison in uræmia was simply urea, but thought that it was either carbonate of ammonia or one of the ammonia series in which carbon enters as an element.—*Lancet, Nov.* 17, 1860, *p.* 488.

34.—CLINICAL REMARKS UPON DIABETES.

By Dr. W. Hughes Willshire, Assistant Physician to, and Lecturer on Medicine, at Charing-Cross Hospital.

[The patient whose case forms the subject of this paper, is a married woman, 29 years of age. Her symptoms began in 1857. Frequent

micturition and intense thirst were as usual the prominent symptoms. A year after the commencement of these symptoms, the sight became impaired, and vision gradually entirely went away. She entered the hospital at first on account of this loss of vision and was under the care of Mr. Hancock.]

We smelt her breath, and noticed that it had a very marked sweet-apple or chloroform-like odour, though not such a strong one as had another diabetic patient of mine up-stairs, the approach to whose bed was something like going near an unstoppered bottle of chloroform. There is no exaggeration in this statement; some of the last session's students will bear me out in what I say, as they were witnesses to the fact.

Now these were general and vital signs, all pointing to the presence of sugar in the patient's urine. Well, then, we examined her urine. We found it voided in quantity between eleven and twelve pints in the day. It was of a very pale straw colour, sweetishly faint in odour, clear, and having a specific gravity of fully 1040. Upon applying the tests of Trommer, Moore, and using the solution of Barreswil, we found the presence of sugar abundantly demonstrated. Some of you will recollect that we afterwards put some yeast to the urine, and so availed ourselves of the fermentation test. This test we also had recourse to again only the other day. It was clear, then, that this woman had glucosuria, and was passing a considerable amount of solid matter in the urine during the twenty-four hours. We examined her lungs, but could not detect that she had that common accompaniment of diabetes—viz., phthisis. But if she was not consumptive, she was *blind*. Mind, this is a point of great interest in this case. She was blind, I say; she had *cataract*, and cataract of both eyes. Before, however, I touch further upon this matter, let me tell you what we did for the diabetes. I ordered her a grain of quinine and a grain of opium three times in the day, an occasional warm bath, and purgatives when necessary. All vegetable matters, except coarse brown bread with the bran in it, and which is made, I may remark, purposely for us by one of our governors, (Mr. Robb in St. Martin's-lane,) were prohibited. A rigorous embargo was laid against sugar, but *four* meat diets, with extra milk and eggs, were allowed her to make up for these denials. Soon after this plan of treatment was put into force, she began to improve; she got much stronger, and the quantity of urine lessened, and the gravity of it became often much lower. She ceased to rise to micturate in the night, the bowels became more manageable, and the thirst much less intense. Sufficient for me to say, she now passes (Oct. 15th, 1860) four pints and a half of urine during the day, the specific gravity of it being from 1038 to 1040. This quantity, with an occasional lower degree of gravity, has been the rule for some months past. Once the quantity went down to three pints, and had a gravity of 1026, but

sugar was still contained in it. Then both quantity and gravity rose again, and do what I may, I cannot get it less than four pints, with a gravity of 1038, for any length of time. The only addition to her treatment up to quite recently has been, that the opium and the quinine were each increased to two grains three times in the day. Once for a few days the patient felt so well, that she could not refrain from eating some potatoes. The urine rose in quantity and gravity, and she had a slight attack of diarrhœa. But she has undoubtedly gained flesh since she has been under the treatment I have mentioned.

Now to revert to her blindness. Your hear the patient had cataract of both eyes, for the relief of which she had sought Mr. Hancock's skilful assistance. Let me now inform you that an American physiologist, Dr. S. Weir Mitchell, published in January of the present year a memoir showing that cataract could be produced in frogs by introducing sugar into their system; and his further experiments proved that a peculiar form of cataract was a pretty constant attendant upon "sugar poisoning." This paper coming across the notice of a gentleman whose genius for physiology is only equalled by his acuteness and persevering energy—namely, Dr. B. W. Richardson,—served at once as a hint to him for undertaking what he has called the "synthesis of cataract." From March last up to the present time, I believe, he has been more or less occupied in its investigation. He first began by introducing syrup-of-cane sugar into the living system of fishes and frogs, and, as a rule, it may be said that "sugar poisoning" and opacity of the lenses were produced. Dr. Richardson then experimented with grape-sugar and with syrup-of-milk sugar, and the like results followed. He then passed on to mannite, and the effects were the same; but in his next experiments with liquorice there was a want of agreement. A grand point now with Dr. Richardson was to be enabled to introduce true diabetic sugar into the living organism. I am glad to think that my colleague, Mr. Hancock, and myself were enabled to place our patient, Phœbe E——, at Dr. Richardson's disposal for this purpose. We had the pleasure, some of you will recollect, of seeing Dr. Richardson at this hospital during the early part of the summer. He obtained from us several pints of the patient's urine, with which, to use his own expression, he "made numerous interesting experiments;" some of these I may add, were performed at Bedford on June 15th, before the members of the South Midland Branch of the British Medical Association. As a rule, again, symptoms of "sugar-poisoning" were produced, and the cataractous condition was "markedly brought out." Other series of experiments have also been continued by him, but for further information about this gentleman's inquiries I must refer you to his papers now publishing in Dr. Brown-Séquard's *Journal de la Physiologie* (No. 12.)

Attention was now directed to the chronicles of *pathology*, to discover if they gave any support to this physiologic view of a causal

connexion between diabetes and cataract. The most concise *résumé* of what is to be obtained in this field you will find in the last volume of Guy's Hospital Reports (Third Series, vol. vi., p. 266), in a paper by Mr. J. F. France. He there tells us that he himself drew attention to "diabetic cataract" in January, 1859, in the Ophthalmic Hospital Reports, but that Dr. Mackenzie, of Glasgow, had alluded to it in 1854. More recently Messrs. Duncan, Wilde, Walton, Veasey, Sloane, Newman, and Barton have instanced cases bearing upon the point. These you will find specifically alluded to in Mr. France's paper. I have referred to Schauenburg's "Ophthalmiatrik" (p. 114); there I find it stated that diabetes is very often followed by soft cataract, and Unger's name is brought forward in support of the assertion. In the *Lancet* of April 28th, 1860, (p. 424), there is a note to the effect that Graefe had stated, in the *Deutsche Klinik*, that diabetes was very frequently the cause of cataract. It is right to mention, also, that Dr. Richardson admits both Dr. Mitchell and himself have been forestalled by Kunde, who some years ago travelled far in the same direction—the synthesis of cataract *(Journal de la Physiologie*, p. 450). With regard to "diabetic cataract," Mr. France generalizes thus: the cataractous condition is symmetrically developed upon both sides; the lenses increase very largely in their antero-posterior diameter; the cataracts are of soft consistence; the ocular affection comes on only after the diabetic state has existed for some time, and there has not in any case been reason to suspect further disease of the eyeball *(op. cit)*.

With regard to our patient, I may remark that Mr. Hancock first operated upon her on June 9th; and he informs me that the cataracts were double and of soft consistency, and that he did not observe that the lenses were larger in any direction than usual. He likewise says that absorption did not by any means proceed so rapidly after the operation as it usually does. The operation performed was that called "breaking up." Mr. Hancock again "broke up" the lenses upon October 8th, and he tells us that although the lens in either eye is not as yet absorbed, the patient is beginning to see very satisfactorily, particularly with the left eye; so that the result of the operations may be considered favourable. Soon after the last operation was performed some inflammation of the left eye occurred, followed by a slight adhesion of the iris. Mr. Hancock and myself had some talk together about suspending the quinine and opium and giving a little mercury. Although not without some misgivings regarding the possible effects upon the diabetic condition, the last mentioned drug was had recourse to, and certainly with benefit to the visual organs. I have been very anxious about the urine, therefore, and have several times examined it. To-day we made some inquiries, you will remember, about this excretion in the wards. The specific gravity of the urine we found to be 1040; the quantity rather more than four pints; and the presence of sugar was indicated by the various tests we employed.—*Lancet, Oct.* 27, 1860, *p.* 404.

35.—ON CERTAIN POINTS CONNECTED WITH DIABETES.

By Dr. FREDERICK W. PAVY, Professor of Physiology and Assistant-Physician to Guy's Hospital.

[It is but ten years since the discovery of a sugar-forming function for the liver was announced. It rapidly became an established doctrine in physiology. At first Dr. Pavy was a strong advocate of this glucogenic theory, attributing a result of his own experiments to some source of objection in the experiment. Other reasons, however, gradually convinced him of the erroneousness of the usually held opinions, being convinced that the liver is not specially intended as a sugar-forming organ. He says:]

Of the varieties of sugar, it is the glucose, or grape-sugar, that we shall have to deal with in these lectures. Now, as it is of the greatest importance that we should possess information about the means adopted for recognising our agent, I will first speak of the tests that are resorted to for determining its presence and its quantity. Happily, here we do not encounter any difficulty, for grape-sugar is almost as easily detected as any inorganic material, even when present in an exceedingly minute quantity. Numerous tests have been from time to time recommended, but the following are those that, on account of convenience or otherwise, are generally used:—

Moore's test consists in treating the specimen with a solution of potash and then boiling. The alkali decomposes the sugar, resolving it into a dark coloured substance, which renders the liquid more or less of a sherry or brownish hue. I do not consider this test a desirable one for physiological purposes. Where the quantity of sugar is large, as in diabetic urine, the effect is so strongly marked that there cannot be any mistake in recognising it; but where only a minute quantity of sugar is present, there is only a shade of difference to discriminate, and, with the best intentions, we may often be deceived, where only shades of difference in colour are concerned. What, in physiology, we require, is something giving a more decided "aye" or "nay," and this we get with the various copper tests, and with fermentation.

In using Moore's test for the detection of diabetes, a drachm, say, of urine is treated in a test tube with about half its bulk of liquor potassæ, and boiled for a couple of minutes over a spirit lamp. It must be borne in mind that a fallacy—and I have frequently seen it—may occur. If the liquor potassæ have been kept in a white or flint-glass bottle, it becomes contaminated with lead. Lead may also be abstracted from the glaze of an earthenware dish, in which the potash may have been boiled or evaporated down. At all events, from whatever source lead may have been derived, on boiling liquor potassæ contaminated with it with any organic compound containing sulphur, the sulphur is liberated, and, combining with the metal, produces a more

or less dark-coloured liquid, according to the amount of sulphuret formed.

Amongst the copper tests we can have our choice; but in all, the principle of action is the same. Free protoxide of copper is present, which, on being boiled with grape-sugar, loses half its oxygen, and is thrown down as a yellow, orange-yellow, or orange-red precipitate, according to the amount of sugar present, which possibly determines the state of hydration of the deposit. A liquid containing a large proportion of sugar gives an orange-red deposit; whilst, when there is only a slight proportion of sugar present, the sub-oxide thrown down assumes a yellow colour.

In Trommer's test, the oxide of copper is set free at the time of application. A drop, or a couple of drops, of a moderately concentrated solution of sulphate of copper are let fall into the specimen to be tested, and then liquor potassæ is added in considerable excess. The precipitate at first thrown down on the addition of the alkali is redissolved if sugar be present, and a deep-blue liquid is the result, which, on boiling, deposits the reduced or sub-oxide precipitate. The objection to this test is, that it is not so convenient in practice as the copper solutions to which I am about to refer; and, where only traces of sugar are present, they may escape detection, on account of undissolved protoxide obscuring a slight production of the reduced oxide.

Of the solutions of copper, Barreswil's liquid is by far the most celebrated. It is this form of copper test that has been employed by Bernard in his researches. Reduced to our English scale of weight and measurement, it is thus composed:—

Bitartrate of potash (cream of tartar)	960 grains.
Carbonate of soda (crystallized) ...	960 „
Caustic potash (potassa fusa)	640 „
Sulphate of copper	320 „
Distilled water	20 fluid oz.

These materials are to be dissolved, mixed, and the resulting blue liquid to be filtered.

Barreswil's liquid is thus a rather complex solution. Essentially it consists of oxide of copper dissolved in an alkali by the presence of an organic material (tartaric acid), which does not occasion its reduction at the temperature of ebullition. It is the property of the protoxide of copper not to be soluble in an alkali unless some form of organic matter be present. Hence the necessity of the tartaric acid, or some such agent, in the cupro-potassic solutions. But why use the bitartrate of potash, which calls for the employment of the carbonate of soda to neutralize its excess of acid? It is more simple to take at once the neutral tartrate of potash, and then the carbonated alkali may be dispensed with. Fehling's solution is of this description, and

consists of sulphate of copper and tartrate of potash, with soda as the alkali.

I can recommend the following as a simple method of preparing a copper solution for use in clinical practice in the detection of saccharine urine. Take five grains of sulphate of copper and ten grains of tartrate of potash, and dissolve in two drachms of liquor potassæ. A clear deep-blue liquid is formed, which is quite as efficient as any other kind of cupro-potassic test. The liquor, however, that I use and keep ready prepared in my laboratory is made with a larger proportion of potash than what is contained in the liquor potassæ—a condition necessary to enable the test to be preserved in an efficient state. The following are the proportions of the ingredients used :—

Sulphate of copper...	320 grains.
Tartrate of potash (neutral)	640 „
Caustic potash (potassa fusa) ...	1280 „
Distilled water	20 fluid oz.

Dissolve each in a separate portion of the water, then mix the sulphate of copper and tartrate of potash, and afterwards add the alkali.

The precipitation of sub-oxide from any of these copper solutions on boiling must not be looked upon as affording an infallible indication of the presence of sugar ; neither, on the other hand, must the absence of a precipitate be regarded as absolutely proving the absence of sugar. Such a remark must seem materially to detract from the value of testing with a copper liquid; but bearing in mind certain considerations, I do not think there is much chance of falling into error, and certainly the copper is the most useful test that we have.

It is said that glycerine, tannine, cellulose, leucine, uric acid, and chloroform are each capable of producing, in different degrees, a reduction of the oxide of copper. Chloroform certainly exerts a strong reducing effect. Uric acid I have observed to occasion some deposit of red oxide ; and from cotton I have obtained just a trace of precipitate. But the glycerine I have tested has not yielded the slightest vestige of reaction. A circumstance, however, of great importance is, that if a copper solution be kept for any considerable period, and particularly if exposed to light, it will of itself deposit some red oxide on boiling. Hence, unless the solution have been recently prepared, it should be tested, by boiling a little, alone, from time to time ; and if it should be found to give a precipitate, a fresh addition of potash will restore it, rendering it again as fit as ever for use.

But under certain circumstances no deposit of sub-oxide will take place, although sugar may be indisputably present. Ammoniacal salts have the property of occasioning this. I have frequently noticed where an ammoniacal odour is evolved on boiling a specimen with the blue liquid, that at first there has been no perceptible change ; then a change of colour, without any precipitation, has taken place ; and if

the boiling has been continued so that the potash of the blue liquid has expelled the whole of the ammonia, a considerable deposit of sub-oxide has afterwards fallen. The presence of albumen interferes with the proper reaction of our copper test: I expect, on account of the ammonia evolved, as the result of destruction by the potash in the test. In a case of apoplexy where the urine was albuminous I tested for sugar, expecting to find it, because there was sudden and great congestion of the circulation. The action of the test was obscure; but on boiling the urine and filtering before applying it, a neat precipitation of sub-oxide occurred.

With a fluid like blood, a process of preparation for testing is of course absolutely indispensable. Boiling does not get rid thoroughly of the albuminous and colouring matters; its alkalinity enables the liquid to retain some of these. The cautious addition of acetic acid to neutralization will effect their entire precipitation; but a little too much acid produces the condition that was intended to be removed. I consider this process troublesome, and always resort to the use of the sulphate of soda—a material which does not in the slightest degree interfere with our test, and which furnishes us with a perfectly clear liquid from the blood. The blood is simply mixed with about an equal weight of sulphate of soda, and heated to effect coagulation in a small porcelain capsule. Thrown on a filter, a limpid transparent liquid immediately runs through.

Bernard has more recently recommended animal charcoal for separating albuminous and colouring matters from a liquid, where we are desirous of applying the cupro-potassic test. According to Bernard, it is the property of animal charcoal to abstract certain materials and not others from a liquid. Glucose is a substance which it does not remove, but the albumen and colouring matters are completely separated by it from the blood, albumen and uric acid from urine, and even the caseine and fatty matter from milk. It is sufficient, therefore, to treat an animal fluid with the necessary quantity of animal charcoal; and upon filtration a clear liquid will run through, which retains any sugar that might have been present, although deprived of many other substances.

A solid substance may be prepared for testing, either by making a plain decoction of it, or else by pounding it in a mortar with the sulphate of soda, heating, and filtering in the same manner as with blood.

Although just a point of doubt might be attached to the indications afforded by the other tests, yet with the fermentation test we have been hitherto accustomed to look upon its reaction as perfectly characteristic of the presence of sugar. It seems, however, from the researches of a French chemist, M. Berthelot, who has recently devoted much time to the chemistry of the sugars, and whose statements are entitled to our greatest consideration, that there are other substances besides sugar—viz., glycerine, mannite, dulcine, and sor-

bine, which are capable of undergoing the alcoholic fermentation with yeast. Alcoholic fermentation, therefore, can no longer be regarded as affording an infallible indication of sugar.

In applying the fermentation test, it is necessary that the yeast should be thoroughly washed before use. Yeast, as purchased, will undergo a considerable fermentation alone, and even after being well washed, I have found it give rise to the evolution of a slight amount of carbonic acid. In fact, the fermentation test in my hands has not proved susceptible of that delicacy and absolute certainty I could have desired. The nature of the gas evolved may be determined by the action of the potash, and the presence of alcohol in the liquid may be shown by distillation, and then mixing the distilled product with unslaked lime, and heating in a convenient apparatus for igniting the vapour that passes over.

It now only remains for me in this preliminary part of my subject, to speak of the mode of conducting the quantitative determination of sugar. The best process I know of is with the copper solution, and this, I am satisfied from considerable experience with it, is susceptible of the utmost delicacy and precision. We cannot separate and weigh the sugar as the chemist does with an inorganic material; but we estimate its amount by its reducing or deoxidizing effect on a copper solution of determined strength. The liquid I employ—that composed of potash, tartrate of potash, sulphate of copper, and water, in the proportions I have already given—is of such a strength, that 100 minims of it are exactly decolorized by half a grain of the purest well-dried grape sugar that I have been able to obtain. In other words, half a grain is the exact amount of sugar required to convert the oxide into the sub-oxide contained in 100 minims of my blue liquid. To give an example, I will describe how I proceed in the case of diabetic urine. The quantities are measured in graduated tubes, drawn at one end to a point so that the liquid may be dropped as required. 100 minims of the blue liquid are taken, and a mixture of one part of urine with five of water (because the urine is too concentrated alone) allowed to fall into it drop by drop whilst kept boiling in a small porcelain capsule over the flame of a spirit-lamp or gas. As soon as the blue colour of the copper solution has been completely removed, the operation is at an end. The amount of diluted urine employed can be read off, and will contain an equivalent to half a grain of glucose.

Should the fermentation-test be used quantitatively, according to Dr. Christison, every cubic inch of carbonic acid evolved corresponds, in round numbers, to a grain of sugar; or, more precisely, 47 cubic inches of gas are equivalent to 45 grains of sugar.

There can be no doubt that sugar is an important material both in regard to animal and vegetable life. It exists most abundantly in the vegetable kingdom, and almost every article of our food derived from this kingdom contains it, in some form and to some extent or other.

Being soluble, and of a highly diffusible character, it readily passes, by a physical process, from the stomach and intestine into the circulatory system. The action of the bloodvessels, in respect to absorption, is purely physical; whilst the special absorbent system of the intestine, as it commences in the villi, has a peculiar selective power, picking up certain materials (oleo-albuminous), and rejecting others, in a manner that cannot be explained by physics. Sugar, salines, and the such-like, in conformity with the laws of endosmose and exosmose, pass into the blood of the portal system, and are carried to the liver before entering the general circulation. It appears that the sugar is arrested in the liver, and converted by it into a material which is found pretty largely in the healthy organ.

In the vegetable world, there is a substance most extensively found, which presents far different properties to sugar, yet is transformed into it with the greatest rapidity when the requisite conditions are present. This substance—starch—is of no use, I apprehend, as such, either to the animal or the plant. It forms a store of carbonaceous material, which may be kept for centuries without undergoing change; and which, on the other hand, is most easily changed when placed in a condition to be made use of in the active processes of life. A grain of wheat, excavated with the mummy belonging to ages past, retains its starch unchanged. Supply this grain of wheat with warmth, air, and moisture, and active life immediately commences. It is now that the starch is transformed; the condition that has created a demand for it, leads to its transformation into a body which is capable of being appropriated to the wants of growth.

Diastase is the principle developed during germination, which occasions the metamorphosis of starch into dextrine and sugar. It seems to me exceedingly doubtful, however, if starch passes on from dextrine into sugar during germination to the extent that is imagined. Certainly the quantity of sugar contained in malt must not be taken as an index of the quantity of sugar existing actually in the germinating grain. I have allowed barley to grow in a temperate atmosphere, fully to the extent that is done by the maltster, and on making a cold infusion of it, have obtained an astonishingly slight indication of the presence of sugar. On the infusion being warmed, or on the growing barley being exposed to an elevated temperature,—as is done in malting on the kiln,—a large production of sugar is the result. It is true, that without the application of heat, when barley has grown to much extent, a quantity of sugar is to be found in it.

In the animal kingdom starch must likewise be transformed before it is fit for appropriation. The pancreatic juice is the secretion which seems specially intended for this purpose. At 90° to 100° the pancreatic juice rapidly converts starch into sugar. This is most easily shown with the secretion obtained from the dog during life. Human saliva converts starch into sugar with an equal rapidity; but it is a curious fact that the saliva of the dog, and of the horse—animals in

which the examination has been made by Bernard;—does not possess a corresponding power. It is a fact that I can attest from my own experience, that the secretions collected from the large salivary glands of the dog, either taken separately or mixed together, have no action whatever towards converting starch into sugar. It appears, however, that the fluid derived from the mouth possesses, to a slight extent, a transformative power.

That it is not the special purpose of saliva to act upon the amylaceous constituents of our food is to be inferred from other considerations besides the difference just alluded to in the extent of power it enjoys amongst different members of the animal kingdom. The starch is seldom ingested as food under such favourable circumstances for transformation as in our operations out of the body where a decoction is employed. The food is delayed but a comparatively short period in contact with the saliva in the mouth, and, supposing any action to take place in this cavity, it must be checked upon the arrival of the mixture in the stomach; for it can be shown by experiment that our saliva exerts no transformative effect in the presence of an acid.

In the case of the pancreatic juice, the conditions are most favourable for the exercise of its transformative power. The food has been reduced to a semi-fluid state, and can be thoroughly incorporated with the secretion that is poured out. The acid of the chyme is neutralized by the alkali of the bile. The temperature is elevated and equable, and the passage along the intestine is slow, so that the starch is delayed in contact with its metamorphosing agent. Probably the secretion from Brunner's glands, and possibly that from the surface of the small intestine, assist the pancreatic juice, which also has an office to perform in connexion with the emulsification or preparation of fatty matters for absorption.

Such are the sources of sugar that were known previous to Bernard's time. Now, after an attentive examination of the circumstances connected with diabetes, Bernard was led to think that there might be something besides amylaceous matter—something unknown to chemists and physiologists, which might give rise to the production of sugar in the body. This he particularly inferred from the fact that the quantity of sugar escaping from a diabetic patient, under a mixed diet, is much greater than can be accounted for by the starch and sugar ingested; and that, although saccharine and amylaceous materials may be completely abstained from, still sugar does not cease to appear in the urine. Here was his motive for undertaking an experimental investigation, out of which have sprung such unanticipated results. In his first experiments, animals were kept upon a diet of a strongly saccharine character, with the view of ascertaining how far the sugar could be followed in the circulatory system, after its absorption into the branches of the portal vein. His results led him to the conclusion that it might be detected as far as between the liver and the right side of the heart. To show that the sugar encountered at this point was what was derived

from the food, a counter-experiment—an experiment where starch and sugar were excluded from the diet—was made, with the expectation of obtaining a negative result. Instead of this, however, to Bernard's great astonishment, sugar was found as before; and thus was laid the foundation of the theory which has given to the liver a gluco-genic function. It was in the year 1848 that the discovery of this function was announced.

Bernard's celebrated experiment, which was regarded as establishing the existence of a sugar-forming function in the animal system, was this:—A dog was kept for some time on a purely animal diet, and was, therefore, prevented from receiving any sugar from an external source. Its life being suddenly destroyed, the abdomen was immediately opened, and a ligature placed on the portal vein. The blood flowing from the chylo-poietic viscera towards the liver was obstructed, and distended the vein below the ligature. An incision into the distended vessel allowed the blood to escape, and this on being collected and tested, gave no indication of the presence of sugar. Blood was now procured from the hepatic veins on the other side of the liver, and was found to be highly impregnated with saccharine matter. The liver tissue itself was examined, and gave a strong reaction, whilst all the other solid organs of the body and the contents of the stomach and intestine gave no reaction with the sugar tests.

Nothing, certainly, seems more conclusive than such an experiment as this. There is no sugar in the blood going to the liver, and there is an abundance in that escaping from it, as there is also in the tissue of the organ itself. Such evidence really appears in unmistakeable language to indicate that sugar is formed by the liver, and carried away from it in the hepatic blood. And, from the extent to which the sugar is found, it might be reasonably inferred that its production is connected with some very important purpose in the animal economy. Now, the experiment establishes beyond all question that sugar can be produced in the animal system without being derived directly from the food; but the great point of interest in physiology, and likewise as regards the pathology of diabetes, is whether this production is really taking place during life, in accordance with the inference that has been drawn from Bernard's results obtained after death. Does, in fact, the examination, as it has been hitherto conducted, afford us an indication of the physiological state? or can it be only taken for what it is really worth—namely, as the representation of a post-mortem condition? This I shall specially touch upon when I have proceeded a little further, and brought the history of the subject up to the present time.

It was at first considered that the sugar found in the system of the animal feeder was formed from an albuminous material. Bernard's next step—and an important one it has proved—was to discover and isolate the substance which is the source of animal glucose. In September, 1855, he communicated to the Academy of Sciences of

Paris his discovery that the formation of sugar in the liver might be shown to be capable of taking place after death. On passing a stream of water through the vessels of the liver after removal, the whole of whatever sugar impregnated its tissue was washed out, so that it then gave no reaction with the copper or fermentation tests. Upon placing it aside, however, and examining it again some time afterwards, a strong indication of the presence of sugar was to be obtained. All question of a special vital process was by this experiment removed, and the production of sugar resolved itself into a simple chemical action. The source of the sugar was not as yet disclosed; but it was evident that in the liver it must be, and that it must consist of something not so easily taken up by water, otherwise it would have escaped from the organ with the sugar. In 1857 the isolation of the sugar-forming material was announced, and as sugar-formation was supposed to constitute its physiological destination, it was called by Bernard the Gluco-genic Matter of the liver. The gluco-genic theory was considered to remain unaltered by this discovery, except in so far as it was rendered more complete by the detection of a recognisable source for the animal glucose.

My researches have led me to look upon the term gluco-genic as objectionable, if applied to this newly-discovered substance upon physiological grounds. It is true that, after death, and under certain unnatural conditions, it is a sugar-forming substance; but under natural circumstances it does not seem intended for the production of sugar. On account of its chemical resemblance to a product of the vegetable kingdom, it has also been called the Amyloid Substance. To this term, however, an objection might be raised on the score of confusing this substance with a body of a different nature met with in the system that has long since received the same name. In speaking of it myself, I have called it Hepatine, simply from its connexion with the liver. When our knowledge of its physiological relations is more perfect, it can then be named upon scientific principles; but until then its provisional name should certainly be one which is not exposed to the chance of leading to error by implying a purpose I have good reason to believe it is not naturally intended for.

The situation of hepatine in the liver is in the hepatic cells—at least, such is to be inferred, from what is seen by the use of the microscope, with the employment of chemicals. As it is not present after death from disease, but is always present under healthy circumstances, its production may be supposed to result from the exercise of the normal functional activity of the liver.

In its chemical properties hepatine is allied to starch, but even more closely so to dextrine. When pure, it is a neutral, colourless, tasteless, and inodorous body, presenting only an amorphous granular appearance under examination with the microscope. It is largely soluble, although not with rapidity, in water; and the aqueous solution presents an opaquely lactescent character. It is curious that in

the form of a highly concentrated solution it is transparent, the lactescence appearing only on dilution. It is insoluble in alcohol and glacial acetic acid, by which agents it may be thrown down from its aqueous solution. It is unaffected at a boiling heat by caustic potash. It is devoid of nitrogen, and is composed of C_{12} H_{12} O_{12}. With iodine its behaviour is like dextrine, producing a deep wine-red coloration. Its most important property is its suceptibility of transformation into sugar. It does not itself react with the cupro-potassic or fermentation tests; but, after boiling for a short time with a mineral acid, or contact with many animal products, as saliva, blood, liver tissue, &c., at a moderately elevated temperature, the characteristic indications of glucose are discernible. For studying its relations, it is easily procured by plunging a piece of liver removed from a recently killed animal into boiling water; then pounding in a mortar, and boiling afresh with a fairish quantity of water. The strained or filtered liquid is to be poured into five or six times its bulk of spirit to precipitate the hepatine, which, after being collected on a filter, is to be dried. To obtain it pure, it must be boiled in a solution of potash. After reprecipitation with spirit, it is to be thoroughly washed with that agent to effect the removal of foreign matter. But traces of the alkali cling to it with great tenacity, and these can only be got rid of by neutralizing with an acid, such as the acetic. The acetate formed is carried away by the spirit in washing.

[It will be observed that Dr. Pavy does not in the least impeach the accuracy of Bernard's experiments, but simply affirms that the existence of a certain state after death, does not prove that this state existed during life. The position of the matter is this:]

Experiments have been performed, and results obtained, about the conclusive character of which there cannot be a difference of opinion. But these results have been derived from *post-mortem* examinations, and taken to represent the *ante-mortem* state. Now, it happens, as can be shown by a different mode of experimenting than that hitherto adopted, that the two do not correspond; and here is my ground of opposition to the gluco-genic theory. A physiological condition has been inferred from the state observed after death, and the inference proves to be fallacious.

The knowledge required is of the state belonging to life, and this we fail to obtain unless we operate on blood, cardiac as well as arterial, that is collected before, or at the very instant of death.

I was so strongly impressed with the notion that the gluco-genic doctrine was indisputably established, that I was for some time disposed to think there might be some fallacy in my experiment, rather than call in question the truth of our views. Seeking in vain for a source of fallacy, I afterwards found that what had been regarded as the natural state of the blood of the right side of the heart was not in reality such; that the blood collected, as had hitherto been done,

after death, did not behave like the blood removed during life; and therefore, that an inference of the condition of the one from the examination of the other must be discarded as erroneous.

When blood is collected from the right side of the heart, as in an ordinarily conducted examination after death, it yields a strong indication of the presence of sugar. In five quantitative analyses I found the proportion of sugar to vary from half a grain to nearly one grain per cent. in defibrinated blood. When collected, on the other hand, during life, under natural circumstances, the amount of sugar is certainly not more than is encountered in the arterial system.

It is not absolutely necessary to resort to the operation of catheterism of the ventricle to obtain a specimen of blood presenting the natural character belonging to life. With the information now in our possession, the experiment may be so conducted as to display the physiological state after death, and I do not know that this is not the most preferable mode of operating, on account of the result being less liable to be influenced by accidental disturbing circumstances. Life is destroyed by pithing, and instantly afterwards the chest is as rapidly as possible opened, the heart seized, and a ligature firmly applied around its base. The blood is then collected from the right ventricle; and if the operation have been expeditiously effected, it will be found as free from saccharine impregnation as if catheterism of the heart had been performed under a tranquil state during life. The object is to collect the contents of the ventricle before the blood has become contaminated with the sugar, which is produced in the liver with such astonishing rapidity after death. After the destruction of life by pithing, the circulation continues for a short time, whilst the process of respiration is instantly stopped. By this continuance of the circulation, the opportunity occurs for the impregnation of the blood with the sugar formed in the liver, as a post-mortem occurrence. Now unless the steps of our experiment be quickly performed, the sugar will have reached the heart, and even have been carried through the lungs into the arterial system. There is nothing to be wondered at in this, seeing how quickly foreign agents introduced into the circulation have been found to be transported from one part of the system to another.—and seeing, as I shall hereafter point out, how quickly the saccharine principle is produced in the liver, as the result of the destruction of life.

As far, then, as we learn from what has preceded, there is not, as a natural process of life, that flow of sugar into the circulation from the liver, for the purpose of destruction in the lungs, which the former mode of experimenting led physiologists to believe. After death, and under certain unnatural states during life, it is true, there is a large escape of sugar from the liver; but, as a normal condition, there is only a trace of sugar in the blood between the liver and lungs, and this trace is also met with on the other side of the lungs, in the blood returning from the system at large, and even in the

blood in its passage to the liver. The blood therefore—namely, that returning from the liver—which was formerly looked upon as affording evidence of the exercise of a gluco-genic function by this organ, has nothing special belonging to it. The same character is met with, as far as I can discover, to a precisely equal extent, in the blood of every part of the system.

Although I had discovered the facts I have pointed out respecting the blood, yet I still continued to look upon the liver as strongly impregnated with sugar during life. Analysis of the organ after death displayed abundance of the saccharine principle, and I had nothing yet before me to lead me to suppose that this was not the true representation of the living state. At a loss, however, to account for the blood escaping from the liver not being charged with sugar during life as it is after death, I began to seek for a cause, and thus turned my attention to the liver itself.

The first idea that suggested itself was, to try the effect of imitating, by artificial means, the circulation under different degrees of tension. For this purpose, defibrinated bullock's blood was injected through the liver at different pressures. Nothing satisfactory, however, was elicited by these experiments.

It now occurred to me to examine closely the chemical relations of hepatine, as far as regarded its transformation into sugar. But, in the first place, knowing that hepatine was convertible into sugar by a kind of fermentation, a foreign substance—a strong solution of sulphate of soda—was injected into the liver instantly after death, to see if it influenced this fermentation, and altered the amount of saccharine matter found. The result obtained induced me to proceed, and led me to consider it possible that the sugar hitherto found in the liver might be in reality due to a post-mortem occurrence, just as in the case of the blood.

For, looking to the rapidity with which saliva acted in converting hepatine into sugar, I could perceive it to be possible that a sufficient transformation *might* be effected, during the interval that elapsed between the period of death and the exposure of the liver to the requisite degree of heat to coagulate it and prevent further change, to account for the sugar found in the tissue of the organ by the mode of examination that had hitherto been adopted. Experiments were now conducted upon a solution of hepatine, making use of saliva as a ferment. Various agents were tried in order to see if anything could be discovered to check the saccharine metamorphosis without destroying either of the principal bodies concerned. The alkalies were found to possess this property; a very small quantity of potash, added to a solution of hepatine, sufficed to prevent the operation of the transformative action of saliva. Upon this fact, I was led to perform the experiment of injecting the liver with potash. The alkali having prevented the action of saliva as a ferment, I thought it would exercise a similar effect upon other ferments, and that by injecting a

strong solution of it through the portal vein into the liver as expeditiously after pithing as practicable, I should meet with an organ, as far as hepatine and sugar were concerned, in the condition belonging to life,—or, at least, much nearer to life than had hitherto been done.—and thus decide the question that was agitating my mind.

Upon this injection of potash being performed, it was in vain I sought for the accustomed behaviour of the liver. I could now satisfactorily account for the difference that had been observed in the state of the blood before and after death. But the fact required collateral proof, as it led to so important a conclusion. That the potash had not destroyed the sugar, but simply prevented its formation, was shown by practising the injection upon a liver that had been allowed to remain a short time after death to permit the production of sugar to take place. The result, under these circumstances, was, that the presence of sugar was as readily discoverable as if the injection of the alkali had not been employed. Another modification of the experiment adopted was, to operate on the liver instantly after death, and to practise the injection on a portion only of it. On analyzing the two specimens thus obtained, the one gave no reaction with the copper solution whilst the other occasioned a strong reaction indicative of sugar.

Acids act in a similar manner to alkalies in checking the transformation of hepatine by ferments. I have employed a strong solution of citric acid for injection into the liver, and have found it occasion an absence of sugar.

There is yet another and a far more preferable process for enabling us to alight upon the natural condition of the liver belonging to life. Conclusions derived from the employment of chemical agents such as acids and alkalies might be regarded with suspicion by some, but the effect produced by the simple influence of a variation of temperature cannot, I think, be open to any source of objection. Now, it happens that either of the two extremes of temperature will answer our purpose. Freezing suspends organic chemical changes of every description, and boiling destroys the ferment without exercising any possible influence upon the hepatine or sugar.

For the application of the freezing method, a mixture of ice and salt is employed. The mixture must be allowed to liquefy before being used, its power of rapidly abstracting heat being then much more perfect. The object is to instantly reduce the temperature to a degree at which the ferment ceases to possess activity. The life of the animal being suddenly destroyed, the abdomen is immediately opened, and a piece of the liver excised as hastily as possible, and plunged into the freezing mixture, in which it is afterwards moved about. In the course of a very short time the liver is frozen quite hard. Being removed, it is cut into thin slices, reduced to a pulp in a mortar, and thrown a little at a time into a small quantity of water contained in a capsule, which is to be kept thoroughly boiling during the process.

If the specimen were allowed to pass through a gradual elevation of temperature to obtain the decoction for testing, the experiment would be vitiated, as sugar would be formed during the process of preparation. The liquid procured is a concentrated decoction of the liver. It contains plenty of hepatine, but gives no indication, or, at the very most, the merest trace, of indication of sugar. With several specimens, where the experiment has been happily conducted throughout, I have not been able to discover the slightest reaction of sugar; whilst the remainder of the liver, which had not been submitted to the influence of cold, but tested in the manner it was formerly the custom to do, gave a copious reaction with the copper solution.

To prove that merely a suspension of change is effected by the freezing, it is only necessary to expose the frozen liver for a short time to a temperature of about 90° or 100°. When prepared for testing, the most abundant evidence of the existence of sugar is to be obtained.

I need scarcely say, that a liver which has been allowed to become saccharine after death loses none of its saccharine quality by immersion in a freezing mixture.

I have conducted several experiments, both during digestion and at a moderate period afterwards, but have failed to discover any difference in the result. I usually operate on the rabbit, because I find, from experience, that the steps of the experiment can be more rapidly carried out; and the liver being thinner, the portion excised is sooner acted upon throughout by the cold. With a thick piece of liver, I have before now noticed, that whilst the outside portion was devoid of sugar, the central part contained it in moderate quantity. It is obvious that, with a thick mass of liver substance, the centre must escape being influenced for a greater or less period. Indeed, I have seen it, after some minutes' immersion, still quite soft.

The application of heat, although acting in a different way, leads to the same result as the application of cold. Instantly after occasioning death, in as speedy a manner as possible a piece of the liver is removed, and plunged into about a quart of water that has been placed over a flame or the fire, and is in a state of active ebullition. The piece of liver should not be too large or too thick, and I usually make a few incisions into it, so that it may be more rapidly acted on throughout. The boiling water coagulates, and thus destroys the ferment; and after a minute or two's immersion, all that is necessary for testing is, to pound the liver in a mortar, to add a little water, and then make an ordinary decoction of it. This process is perhaps not quite so efficient as that of freezing. It will be easily understood that every degree of elevation of temperature, until the point is attained at which the transformative agent loses its activity, is more and more favourable to the production of sugar; so that, unless the arrival at the requisite degree of heat be almost sudden, the object we have in view escapes being attained. On following, however, the directions I have given, speci-

mens are easily obtained, yielding only a barely recognisable indication of the presence of sugar.

In these experiments upon the influence of temperature, as in those with the injection of an acid or alkali, everything depends upon the rapidity with which the required condition of the liver is produced after the destruction of life has been effected. The transformation of hepatine into sugar takes place so immediately after death, in the liver of the warm-blooded animal, that unless the greatest promptitude is exercised an unsuccessful result is sure to be obtained.

Amongst cold-blooded animals the operation is easy enough for procuring a representation of the natural condition of the liver. According to the lowness of the temperature, the post-mortem change is proportionately retarded. Hence, with ordinary precautions, especially during cold or moderately cool weather, the liver of a cold-blooded animal is readily alighted upon in a state that belongs to life.

The frog is an exceedingly convenient and suitable animal for experimenting on, for this, as for so many other purposes. When it is in good condition, the liver is large, pale-coloured, and abounds in hepatine. For an examination four or six livers are sufficient. The animals are killed, and the livers excised and plunged into boiling water. A concentrated liver decoction is then made, which is opaquely lactescent from the quantity of hepatine it contains. Boiled with the copper solution, it gives no reaction of sugar. If the livers, however, are kept for some time, or exposed for a short period to moderate warmth before preparation for testing, then a strong reaction of sugar is obtained. Also, if the experiment is conducted when, from an artificial cause or otherwise, the temperature of the animal is at a rather elevated degree, a small amount of sugar will be found, unless the steps of the operation have been expeditiously performed, in order to prevent any post-mortem transformation occurring.

The influence of temperature on the liver of a frog has not escaped the observation of Bernard. My own knowledge was obtained from independent observation; but in 1857, Bernard in a communication to the Academy of Sciences, mentioned that lowering the temperature of frogs caused a disappearance of sugar from the liver, whilst a reappearance took place on afterwards exposing them to heat. He added that it was possible to produce this singular alternation of appearance and disappearance of sugar several times without any food being given, and by acting solely on the phenomena of the circulation through the medium of the temperature. Such was his explanation of this phenomenon—namely, to refer it to an alteration in the activity of the gluco-genic function, brought about by increasing or diminishing the activity of the circulation. My own explanation has to be inferred from what has preceded. It depends simply on the influence which a high or a low temperature exercises over the rapidity of the post-mortem production of sugar.

The oyster and the mussel are animals which likewise serve well to

illustrate the true state of the liver in regard to sugar formation. In both the liver is charged with a very large quantity of hepatine, but is entirely devoid of sugar when the animal is taken for examination in a fresh and healthy condition. Keep it and allow it to die, and sugar is immediately discoverable in abundance. Also, on exposing the healthy ex-saccharine liver to an elevated temperature for a few minutes, sugar is produced in large quantity.

In the mussel (*Mytilus edulis*), besides hepatine being contained in the liver, the mantle is exceedingly rich in an identical material under a good conditioned state of the animal. When the animal is poor, as after spawning, the mantle is thin, transparent, and watery; but at other times it is thick and opaquely white or yellowish, and, in this state, is highly charged with the amyloid substance, which thus seems to form a store to be drawn upon when required. No sugar can be detected in the mantle at the time of death; and, what is more, there is scarcely a proneness to the production of sugar after death, unless a ferment, such as saliva, be added, when a copious formation of sugar is the result.

There are means for placing a warm-blooded animal in the same position as our frogs in the experiments to which I just now referred. After division of the spinal cord, as high up towards the brain as is consistent with the persistence of life—that is, just below the origin of the phrenic nerves, the temperature of the body rapidly falls if the animal be placed in a cool situation. Now, Bernard noticed that under these circumstances the liver was found devoid of sugar when examined immediately after death, but became strongly saccharine afterwards. He endeavoured to account for this phenomenon under his gleuco-genic theory; but the different reasons assigned show the difficulty he was under in giving a satisfactory explanation of it. The fact itself I have often verified, and the correct interpretation of it, I conceive, consists in the lowness of temperature at the time of death diminishing the rapidity of the post-mortem formation of sugar, and so producing the condition noticeable in the cold-blooded animal. Maintaining the temperature of the animal after division of the cord, by immersion in a heated atmosphere, causes the liver to behave just as if no division of the cord had been effected. Circumstances now exist, as in the warm-blooded animal under an ordinary condition, conducive to the almost instantaneous production of sugar after death.

Again, on oiling the coat of an animal so as to favour the rapid escape of heat from the body, the temperature by exposure to cold may be made fall until it becomes too low for the persistence of life. By this process, the same results are obtained on examining the liver as when the temperature has been reduced by division of the cord.

Facts such as these I have now mentioned show that the saccharine state is not natural to the liver, as has hitherto been supposed in accordance with the doctrine of gluco-genesis. But there is another aspect concerning the liver which remains to be considered. Hepa-

tine is formed by the liver from sugar. Now, that hepatine should be intended to come back again into sugar, certainly seems, to say the least, improbable. The fact taken alone does not of course actually prove anything against the gluco-genic theory; but such a process as the conversion of sugar into hepatine, and hepatine back again into sugar, certainly does not accord with what we should expect from the notion we possess of the manner in which the operations of nature are conducted.

In eleven examples, taken indiscriminately, the average relative weight of the liver in a dog, fed upon a purely animal diet was as 1 to 30. The amount of hepatine given as the average of my analytical examination of the livers in these cases was slightly over 7 per cent.

Under a vegetable diet the five dogs taken gave me the proportion of 1 to 15 as the relative weight of the liver. The organ was thus exactly double the size of that of the animal feeder. Three of the livers were analyzed quantitatively, and the average amount of hepatine given was over 17 per cent.

In four examples where animal food, with a large admixture of cane-sugar, was given, the average relative weight of liver was as 1 to $16\frac{1}{2}$. The analyses in four cases yielded 14·5 per cent. as the average amount of hepatine present.

The experiments made on rabbits have most fully confirmed these results obtained upon a dog.

In one experiment a couple of full-grown animals were taken which as closely as possible resembled each other. One was kept fasting, whilst to the other was administered an ounce of starch, with three-quarters of an ounce of grape-sugar per diem. On the fourth day both rabbits were killed, and the following is the result of the examination made:—

	Weight of Animal.	Weight of Liver.	Amount of Hepatine in the Liver.
Rabbit fasting ...	3 lb. 1 oz. ...	1, say $\frac{2}{5}$ oz. ...	1·3 per cent.
Rabbit fed on starch and grape-sugar. ...	3 lb. 4 oz. ...	$2\frac{4}{5}$ oz. ...	15·4 „

In a second experiment two half-grown rabbits were taken. One, again, was made to fast, whilst to the other was given an ounce of starch with a similar quantity of cane-sugar daily. On the fourth day the animals were killed, and an examination made. The following is the result:—

	Weight of Animal.	Weight of Liver.	Amount of Hepatine in the Liver.
Rabbit fasting ...	1 lb. 14 oz. ...	1 oz. ...	1·4 per cent.
Rabbit fed on starch and cane-sugar ...	1 lb. $14\frac{3}{4}$ oz. ...	$2\frac{3}{8}$ oz. ...	16·9 „

In another experiment a rabbit was allowed to take its ordinary food, and, in addition, three-quarters of an ounce of loaf-sugar and half an ounce of starch were administered daily. The rabbit was killed on the fourth day, and the hepatine amounted to 22·7 per cent., notwithstanding the analysis of the liver was not made until the day after death, when more or less loss must have taken place from a transformation into sugar going on.

All these results are so striking and so corroborative of each other, that it seems to me nothing further can be required to prove that the sugars and starch are made use of by the liver for the production of hepatine. The marked variation observed in the size of the liver under different diets is a point of much physiological interest, and possibly may prove of pathological importance also.

[Dr. Pavy is convinced that sooner or later the present gluco-genic theory must be abandoned. The theory is founded upon the condition of the liver and blood, discoverable by an ordinary examination after death. But the result of such an examination is not in accordance with the true ante-mortem state.]

Briefly to consider them, these are the grounds that have led me to deprive the liver of the special sugar-secreting function which has lately been attributed to it. The organ itself is found free from sugar, or at the most is only impregnated with it to the minutest extent, when treated so as to come down upon it in a condition the nearest possible to that of life. The blood, under normal circumstances, merely contains more or less evident traces of sugar; and the blood in different parts of the system does not present, as far as I can discover after adopting the necessary precautions in the examination, any appreciable variation in constitution. In some recent most carefully conducted examinations, I have been unable to distinguish, by any difference in behaviour, the blood of the portal vein from that of the right side of the heart. I have sought for but been unable to obtain, evidence of the susceptibility of sugar to undergo destruction, to any extent, in the circulation. The result of my experience decidedly is, that if sugar escape from the liver, it is to be detected alike in every part of the circulation; and there is never any extent of sugar in the blood without its being immediately drawn upon and separated by the kidneys. This being the case, the object of sugar formation becomes a most fruitless one; and it is not, I think, to be conceded as probable that the liver should produce it for the kidneys afterwards to get rid of it.

It is possible that, without being susceptible of appreciation by our tests, there may be a slight amount of transformation of sugar into lactic acid going forward in the blood as it is circulating though the different parts of the system; for if saccharine blood be allowed to decompose, the sugar, when decomposition has fully set in, gradually disappears, and the blood becomes acid, which is the reverse of what

takes place when a specimen has been taken that is free from sugar. The changes of decomposition, I apprehend, have excited the lactic acid fermentation precisely as in the case of milk that has turned sour. Now, I see no reason, although I can bring forward no actual proof in support of it, why the changes going on in the blood during its circuit through the body may not act in the same way as the changes of decomposition, and occasion a slow production of lactic acid from the traces of sugar that I have mentioned as being found in every part of the circulation. Whatever, however, may occur of this kind, must be of an exceedingly trifling nature, for the disappearance of sugar is not to be detected in any particular portion of the system by a difference in behaviour of the blood.

To proceed with the arguments that oppose the gluco-genic theory: We have evidence that the so-called gluco-genic matter is actually formed in the liver from sugar as one of its sources. Now, if sugar were its natural destination, the process would be sugar into glycogene, and glycogene back again into sugar—a repetition that certainly appears upon the face of it to be extraordinary and improbable as a designed occurrence of nature.

Again, if there were the constant production and flow of sugar from the liver through the hepatic veins, as is implied under the gluco-genic doctrine, on interrupting the principal stream of blood through the organ, it ought certainly to be expected that less sugar should escape, and less sugar be found in the circulatory system. Yet, as the result of actual experiment, precisely the reverse is the case. A ligature applied to the portal vein causes the blood circulating through the system to present a strongly saccharine character. Notwithstanding the current is so reduced which is supposed to be naturally carrying sugar into the general circulation as a part of a particular functional operation, the amount of sugar now reaching the circulating current is actually very greatly increased. The fact appears to me to be, that the ligature of the portal vein places the liver in an unnatural condition; its functional working does not proceed, with a supply only of arterial blood, in the manner it does when there is also the stream of portal blood to exercise its influence. As the result of this, the hepatine descends into sugar, like it does under many other unnatural states of the liver.

Ligature of the hepatic artery as well as the portal vein causes the liver to become strongly impregnated with sugar during life, as is the case with ligature of the portal vein only; but the blood belonging to the general circulation remains uninfluenced. The flow of blood through the liver being entirely prevented, no opportunity is afforded for the passage of the sugar into the system.

If I have concluded, as my experiments have led me to do, that sugar is not the natural product—that sugar is not specially intended to be formed as the result of the natural transformation of hepatine in the liver, it may reasonably be inquired of me, what is the material

into which the hepatine is really destined to be transformed? It is generally more easy to say what the nature of a particular operation does not, than to pronounce what it does consist of, when our knowledge regarding it has not been completely made out. I do not wish, however, to avoid this question; indeed, I have it constantly before me. But, as I am not in a position to give what I consider a sufficiently satisfactory solution at present, I must content myself by saying, that I have grounds for believing that the product of the normal transformation of hepatine, instead of escaping through the hepatic veins into the circulation, passes down through the biliary passages into the intestine. I am actively engaged in working out the connexion of the liver with the production of fat—a statement that may serve to give a hint of the direction in which my views are tending with regard to the object of hepatine.

In some other parts of the body besides the liver, there is to be found a material which has characters identical with hepatine. Now, if hepatine give rise to sugar in one situation, why should it not do so in others? and why should there not be in the muscles and the cells of the cataneous, intestinal, respiratory, and genito-urinary surfaces of the fœtus, and in the muscles and lungs of the adult, a gluco-genic function?

It is not necessary that the liver should enjoy a special sugar-forming function to account for the presence of that minute trace of sugar which is met with under natural circumstances in the circulation. The production of sugar may result from the operation of a chemical tendency which hepatine happens to possess, and which appears to have no connexion with its physiological destination. When chemistry alone comes into play, as after death, the tendency of hepatine to undergo a saccharine metamorphosis is allowed to take effect; but normally, during life, this mode of transformation is held in check by a force or condition peculiar to the living and healthy liver. There is the material which is so susceptible of undergoing change into sugar, and there is, also, the ferment or agent for exciting it. Both are present in the liver, and yet the process escapes being carried out, except to the most infinitesimal extent. Although I cannot precisely account for this, nevertheless the fact remains the same; and we are not altogether unprepared for it, as we have long been familiar with a parallel in the phenomenon of the coagulation of the blood. Whilst circulating in the living body, the fibrine exists in a fluid state, but with an astonishing rapidity assumes the solid form after the blood is withdrawn. It is the property of fibrine to have a strong tendency to coagulate. As long as it remains under natural circumstances in the living body, this tendency is held in check, but immediately it escapes from the influences thus naturally controlling it, its coagulating property comes into play, and a solidification is the result. Even here, although the phenomenon has been so long familiar to us, it has not yet received what can be considered as a satisfactory and sufficient

explanation. There is, then, nothing supernaturally wonderful, nothing unparalleled, in the position of hepatine before and after death —namely, as to its resisting during life the exercise of its chemical tendency to transform into sugar, and a transformation taking place immediately the controlling influence of life is removed.

I imagine that under the existence of natural circumstances the circulation takes place through the liver without disturbing the hepatine contained in the hepatic cells further than just the minutest trace of it passing into the bloodvessels, and thus accounting for the trace of sugar constantly met with in the different parts of the circulatory system. Hepatine cannot be mixed with the blood without being transformed into sugar. The blood, whether circulating in the body, or experimented with out of the body, acts like saliva as an energetic ferment in converting hepatine into sugar. The injection of hepatine into the circulation of the living animal occasions a saccharine state of the blood, and if enough have been employed, the urine is also impregnated with sugar. This is shown by the specimen of urine before me, derived from a rabbit, in which fifty grains of hepatine were introduced into the jugular vein. It gives the characteristic orange-yellow reaction with the copper solution. Hence it is only necessary for hepatine to pass from the liver cells into the blood, and a saccharine state of this fluid is the immediate result.

The consideration has not been overlooked, that although the quantity of sugar found in the blood of the right side of the heart is extremely small, yet, supposing this small quantity to be constantly escaping fron the liver, it would form a considerable amount in the course of the twelve or twenty-four hours. The sugar, however, small in quantity as it is in right-ventricular blood, has not just been poured into the circulation from the liver. It must be remembered that the blood flowing towards the heart through the general systemic veins, contains sugar, and as far as I can learn from the reaction of our tests, to about the same extent as the cardiac blood itself. So that sugar once in the circulation circulates over and over again, and it would be an error to calculate, from the amount found in the heart and the quantity of blood passing through the liver, the extent of sugar production in a given time. Even, as I have stated, the blood before entering the liver is charged with sugar, and in experiments, where the greatest care has been exercised, where every precaution has been taken to obtain the specimens in the state naturally belonging to life, there has been no appreciable difference of reaction discoverable between the portal and the cardiac blood.

It is interesting to notice how hepatine, in the feeble property of diffusibility it enjoys, presents us with an exemplification of the harmonious adaptation that exists in the exercise of the operations of nature. Looking at the hepatine as contained in the hepatic cells, and reflecting on the close proximity that the blood must hold to it in circulating through the liver, it may be reasonably inquired how it

is that the hepatine does not pass, as it might be expected to do, by diffusion into the bloodvessels—a circumstance that would conduce to render the blood extensively saccharine. Now, it happens, as I have discovered, as can be seen by the experiment before me, that hepatine is the antithesis to sugar as regards extent of diffusibility. If sugar were contained in the liver cells, the circulation could not take place through the organ without largely carrying it away, on account of its being so readily diffusible through membranous septa. With hepatine, on the other hand, there is a resistance to traverse animal membranes. A solution of sugar and hepatine placed together on one side of an animal membrane in an endosmotic apparatus, the sugar diffuses, leaving the hepatine behind. I have even found that under the pressure of a six-feet column of fluid, hepatine has resisted passage through the pores of a piece of bladder. It is on account of this low diffusibility, I presume, that we experience the difficulty we do in filtering a solution of hepatine. The coarse pores of an ordinary filter seem, indeed, to offer some amount of obstruction to the passage of this principle. That hepatine *can* pass through a membranous septum is, I think, sufficiently proved by the fact, that in injecting a stream of water through the vessels of the liver, the hepatine is gradually washed out. Under this operation the liver becomes unusually swollen and œdematous, and its cells must be submitted to a great amount of pressure.

Albumen is another material which, like hepatine, enjoys a low diffusibility. When a solution of it is submitted to experiment, it does not pass through a membrane, such as a piece of bladder, except under the influence of pressure. With a column of fluid, three feet high, the albumen passes, but not at all when there is no pressure employed. This property of albumen is doubtless connected with its retention in the bloodvessels. It is not intended that a material so necessary as a pabulum for the nutrition of the body, should have a tendency, like urea and sugar, to escape from the blood by virtue of the simple physical property of diffusibility. For a corresponding reason, I apprehend, hepatine agrees with albumen, and is not endowed with a tendency to diffuse, because it is not intended to be drawn by the blood out of the hepatic cells.

Looking, then, to this physical character which hepatine possesses, I think it must be admitted that the explanation is clear, why, under natural circumstances, it resists, as it does, transudation into the circulating current, where, being transformed into sugar, it would prove, if not entirely, certainly in great part, useless to the system; for, as I have before pointed out, sugar cannot exist to any notable extent in the blood without being recognisably eliminated by the kidneys. I should say even, that the trace of sugar which is natural to the blood throughout the body, is constantly being drawn upon by the kidney; but that the amount for removal is so small, that it is not susceptible of detection in the urine. Did we possess more per-

fect means of detection than we do, I imagine a minute trace of sugar would be reckoned as a normal constituent of the urine. I am speaking now a little problematically, but what I have said accords with a principle that is being forcibly recognised in analytical chemistry —namely, that the minuteness of diffusion of materials through the different constituents of the universe exceeds or surpasses our powers of detection.

That hepatine can, under certain circumstances, pass from the liver into the bloodvessels, has been proved by what I mentioned to be the effect of injecting a stream of water through the organ after death. Now, may it not be that we have here an illustration of the manner in which saccharine urine is occasioned in some instances? Violent muscular efforts, by which the liver is compressed—and obstruction to the breathing, by which the circulation is interfered with and congestion produced—will occasion a highly saccharine state of the blood. In the case of the latter, by keeping up the respiratory disturbance I have rendered the urine strongly saccharine. Both disturbances, as far as the liver is concerned, are of a physical character; and is it not purely to physical circumstances that the unnatural condition observed is attributable? When the liver is compressed, a favourable condition is established for promoting the escape of hepatine; and when it is congested, the hepatic cells being, I conceive, submitted to compression, the condition will, in a similar manner, be favourable to a transudation of hepatine, and its admixture with the blood. In the human subject, saccharine urine has been noticed in cases attended with an obstruction to the flow of blood towards the heart. For example, it has been observed in whooping-cough, coma, and pneumonia; and I look upon it here as entirely referable to a physical alteration of the conditions in the liver. In whooping-cough the protracted paroxysms of coughing not only occasion a great amount of venous congestion, as is indicated by the countenance of the patient, but the liver must, at the same time, be compressed externally by the violent action of the abdominal muscles. In coma, with stertorous breathing, the impediment to the circulation of blood through the lungs produces general congestion of the system, and in this the vessels of the liver must participate. A case of this kind only recently fell under my notice. A man, one of the carpenters of our hospital, suddenly fell down in the street in a fit of apoplexy. As there was great lividity of the face, with slow, stertorous breathing, I suggested, about eighteen hours after the fit had occurred, that the urine should be tested for sugar. It was highly albuminous, and gave, after the albumen had been coagulated and separated, a copious reaction with the copper solution. There was no evidence that this person had saccharine urine previous to the occurrence of his apoplexy. In pneumonia, where a large portion of the lung is rapidly involved, the patient is likely to suffer from great dyspnœa; and thus, through the circulation being influenced, saccharine urine may be occasioned at the commencement of the disease.

After division of the pneumogastrics in the lower animals, I have often observed the production of saccharine urine. It depends upon the activity of the respiratory function being so diminished as to offer an impediment to the natural flow of blood through the system ; and it is only where this is prominently the case that sugar is to be recognised in the urine.

The theory founded on the doctrine of gluco-genesis referred diabetes either to a production of sugar in excess of its capacity for destruction, or, the production remaining the same, to an interference with its normal extent of disappearance. The production of sugar specially for subsequent destruction in the system must, if what I have brought forward prove substantial and true, be now left out of sight. There ought to be no sugar of any consideration in the blood, and, should there be any, it may be considered as resulting from some unnatural circumstance leading to its production. I have pointed out how I consider that a physical disturbance of the circulation may occasion a flow of hepatine into the blood, and so lead to the presence of sugar in the urine. The same occurrence may I think, arise from an alteration of the natural forces in operation in the liver. Suppose, for instance, that the chemical tendency of hepatine is allowed to come into play, as is the case after death. then sugar will be formed in the liver tissue, and diffuse itself into the contents of the bloodvessels; it is only necessary for the circulation to be kept up, and a saccharine quality of the urine will be produced. On destroying, for example, the life of an animal, and adapting a contrivance for maintaining artificial respiration in a regular and efficient manner,—which I effect in my laboratory by means of a double pair of bellows connected with the trachea, and so arranged that in working, one pair forces the air into the lungs, whilst the other afterwards draws it out,—the action of the heart continues, and the sugar which is formed as the result of a post-mortem occurrence in the liver, is distributed by the circulation throughout the body, and being thus carried to the kidneys, is in part eliminated with the urine. One hour I have found to be quite sufficient to give to the urine a highly saccharine character in this way.

The method employed for destroying life in obtaining the result I have just mentioned, has been by pithing the medulla oblongata. As far as my observation has gone, it does not signify what particular part—upper, lower, or middle—of the medulla oblongata forms the seat of injury or division, the result in each case is the same. But the experiment has been also attended with a similar production of saccharine urine when life has been removed by poisoning with agents capable of acting rapidly through the cerebro-spinal system. Bernard has stated, and my own experience confirms the accuracy of the statement that after killing with the woorali poison, if the circulation is kept up by the performance of artificial respiration the urine is rendered strongly saccharine. I have also found that in substituting strychnine for the woorali, the effect is perfectly identical.

Although division of the medulla oblongata leads to the production of saccharine urine when the circulation is kept up artificially; yet such effect is not observed after division of the spinal cord. I have repeatedly divided the spinal cord, and without producing diabetes. I have gone as high as between the second and third cervical vertebræ, after which the employment of artificial respiration is required to maintain the circulation, as after division of the medulla oblongata.

Again, the brain does not seem to exercise the same influence over the state of the liver as the medulla oblongata. It is true, I am not enabled to speak in quite such positive terms with respect to this organ; but my experiments induce me to believe that its functions may be destroyed without placing the liver in the condition noticeable after actual death, or after division of the medulla oblongata. In operating on the brain, circumstances may arise to complicate the result. Where, however, there has been the least complication, I have observed that the brain may be completely separated from the medulla oblongata without occasioning a saccharine state of the urine. In the last experiment I performed in reference to this point, the effects were as simple and the result as conclusive as any I could hope to obtain. The crura cerebri were divided just in front of the pons Varolii, so that the cerebrum was completely separated from the other parts of the encephalon. The breathing continued without assistance. It was quick and short, and frequently attended with a sigh. The heart's action was extremely rapid. The urine remained devoid of sugar.

The natural inference to be drawn from what I have been stating is, that the cerebro-spinal system gives to the liver during life a force or a condition which it does not possess after death, and that the part of the cerebro-spinal system particularly connected with this object is the medulla oblongata. In Bernard's celebrated experiment of puncturing the fourth ventricle for producing diabetes, the effect was supposed to depend upon a stimulation of the nervous centre. I cannot now coincide with this. My opinion is that the diabetic effect following the puncture of the medulla oblongata is produced in the same way as that following the destruction or division of the same part. I regard it as due to a loss of the proper nervous influence. In the one case the lesion is not sufficient to check also the respiration; in the other it is, and here the breathing has to be maintained artificially for the effect on the urine to become manifest.

Looking upon the medulla oblongata, then, as a centre, either directly giving to the liver a force capable of preventing the saccharine metamorphosis of its hepatine, or possibly indirectly maintaining through its influence on other organs such a state of the portal blood as is conducive to the fulfilment of this end. I determined to look for the channel through which its action might be exercised. It was working with this view before me which brought me to discover that injuring certain parts of the sympathetic system most rapidly occasioned a strongly saccharine state of the urine.

Supposing a force to start from the medulla oblongata for the object I have mentioned, I premised that its transit to the liver could not be, at least, exclusively through either the spinal cord or the pneumogastrics, as division of each had been frequently practised without any diabetic effect being produced. But the result of dividing both cord and pneumogastrics had not, as far as I was aware, been ascertained. I therefore started with the performance of this operation, which I found, however, was unattended with the appearance of sugar in the urine. In the experiment that was performed, the division of the cord was effected between the third and fourth cervical vertebræ, so that artificial respiration had to be resorted to.

Although no diabetic effect was observed after division of the cord and pneumogastrics together, yet, after division of everything belonging to the nervous system in the neck, as by decapitation, the urine became in a very short time strongly saccharine, the circulation being kept up by the performance of artificial respiration.

The result in these experiments tended to confirm the notion I had commenced with. Complete separation of the medulla oblongata from any possible connexion with the liver produced a saccharine state of the system, whilst division of the cord and pneumogastrics did not. But might not the sympathetic which also runs through the neck, form the medium of communication sought for? This was the next question that naturally suggested itself, and had to be answered by experiment.

Division of the carotid sympathetic—that portion of the sympathetic system which passes down from the superior cervical ganglion in front of the vertebral column towards the chest—is an old experiment, and one that is often repeated by physiologists on account of the interest it has in connexion with the production of heat. It does not, however, occasion any perceptible effect on the urine. But there is yet another portion of the sympathetic system running through the neck, and this, previous to the prosecution of the present inquiry, has, so far as I know, enjoyed immunity from attack by the scalpel of the experimentalist. This, however, consisting of the filaments accompanying the vertebral artery, might constitute the channel I was searching for. I therefore determined to effect a division of it, and my first experiment resulted in rapidly occasioning a highly saccharine state of the urine. Thus consisted the origin of my experiments on the sympathetic in connexion with the production of artificial diabetes. Such was also the process of reasoning that led me to this point. The evidence afforded by certain experiments had induced me to believe that the medulla oblongata in some way presided over the functional activity of the liver, so as to overcome the tendency to the production of sugar in concurrence with the operation of the unopposed forces of chemistry. I thought there must be a channel of nervous communication between centre and organ, which, if interrupted, would place the liver in the same condition as it is after death; and, on experi-

menting, I found that dividing the vertebral portion of the sympathetic seemed to occasion this. I at once naturally concluded that everything had now become clear. The fact, indeed, that lesion of the vertebral filaments of the sympathetic produces saccharine urine, is indisputable. This certainly is the truth, but it is not the whole truth. There remain behind certain other facts in connexion with the sympathetic which show that our knowledge is still incomplete—which cast a shadow of uncertainty over the notion that I first looked upon as so clearly made out. Lesion of many other parts of the sympathetic, besides its vertebral filaments, occasions saccharine urine; and I really must admit that I am scarcely in a position yet to give a satisfactory explanation of the phenomenon. I will, however, furnish a summary of the results that have attended my experiments on lesion of the sympathetic in different parts of the body; it being understood, unless otherwise stated, that the animal experimented on was the dog.

Division of the vertebral filaments of the superior thoracic ganglion just before entering the vertebral canal on both sides of the neck, has given rise to urine, in *half an hour's* time, so highly charged with sugar as to produce a copious yellow reaction with the copper solution. On the following day the urine has lost its saccharine quality, andon the day succeeding this, the animal has died.

In another experiment, the filaments, on one side of the neck only, were at first divided. Up to *an hour and a half* after the operation, the urine gave only a trace of saccharine reaction. The filaments on the other side of the neck were now divided, and in *half an hour's* time the urine was intensely charged with sugar. Death took place on the following day. There was no urine in the bladder to test; but the liver was found entirely free from hepatine and sugar.

Although in these experiments on division of the vertebral sympathetic before entering the vertebral canal the pleura is not opened, yet the most intense pleuritic inflammation is set up, which has always occasioned a fatal termination. The inflammation evidently results from injury to the sympathetic, because, when, owing to a difficulty in seeing them, it has proved that I have failed in dividing the filaments sought for, there has been no pleurisy set up.

I have followed the vertebral filaments upwards, and divided them in different parts of their course through their canal. A curious circumstance is here to be noticed. In my first experiments, as I thought it useless to attempt to tie the vertebral arteries after entering their canals, and as I could not isolate the sympathetic so as to effect its division without dividing, also, the associated vessels, I considered it best to ligature the two vertebrals before reaching their canals, and, likewise, the two carotids, on account of the freedom of anastomosis existing above. It might be imagined that on suddenly arresting these four streams to the head, considerable disturbance of the cerebral functions would be produced. Such, however, is not the case; indeed, scarcely any visible effect is produced. Now it happened,

where these vessels had been tied, that on afterwards cutting or tearing through everything in the vertebral canal on each side of the neck, a strongly saccharine state of the urine was rapidly induced. For the production of this effect, however, the deligation of the arteries is an indispensable part of the experiment, although when this operation alone is performed, no diabetic result is produced. The conclusions furnished by my experiments on this point stand thus: Careful deligation of the two vertebral (below their canal) and two carotid arteries does not occasion saccharine urine, nor does simply tearing through everything traversing the vertebral canal on either side of the neck—an operation that I find may be performed without any material loss of blood being occasioned; but when these operations are combined, sugar rapidly appears in the urine. I must confess I do not precisely see the *rationale* of this, but it was some time before I could even make out anything at all definite on the subject.

Although division of the carotid portion of the sympathetic in its course through the neck does not produce saccharine urine, yet removal or injury of the ganglion above—the superior cervical ganglion—occasions in a very short period a most strongly marked effect. The exposure of this ganglion, after a little experience, is an operation that is easily performed, and nothing of importance besides need be disturbed. In one experiment, I removed the ganglia of the right side only at first. The urine collected in an hour's time was intensely saccharine. I may observe, as an effect of the operation also, that the ear on the right side was rendered 3° (Fahr.) warmer than the other, the pupil more contracted, and the nostril drier. On the following day the urine was still saccharine. The next day, however, the sugar had disappeared, and on the day succeeding this, the urine not being saccharine, the ganglion of the left side was removed. In half an hour's time the urine had again become strongly charged with sugar.

The following is the amount of sugar found upon analysis in two experiments, where both ganglia were removed at once. In one, the urine half an hour after the operation contained 20·5 grs. of sugar to the fluid ounce, and two hours after, 11·4 grs. In the other, 22·86 grs. per ounce were given one hour and twenty minutes after, and 34·08 grs. half an hour later.

I have conducted several experiments upon the sympathetic system in the chest. Division on one side has sometimes produced as rapid and strongly marked a diabetic effect as has occurred after the other lesions of the sympathetic to which I have referred. At other times, however, there have been only traces of sugar discoverable in the urine, and at others times still, none at all. Precisely the same remark applies to division of the sympathetic on both sides of the chest. Seeing this diversity in the results here obtained, I have purposely operated on different parts of the sympathetic trunk in its course through the chest, but have failed in finding anything connected with the mode of performing the experiment to account for the variety observed.

In the case of the rabbit, diabetes is not so readily established by injuring the sympathetic as it is in the dog. The superior cervical ganglia are conspicuous bodies and easily reached. Their removal in the healthy and vigorous animal occasions saccharine urine, although the effect is not produced with anything like the rapidity that I have noticed in the dog. I have tried to divide the sympathetic at the lower part of the neck, but have met with difficulties in the performance of the operation arising from the animal being so easily killed, and the nervous filaments being so small and difficult to find, that I do not yet possess a record of a fair experiment to speak of.

The diabetes resulting from all these operations on the sympathetic is quite of a temporary character. What the chain of phenomena is that determines the effect constitutes the important problem that yet presents itself for solution. I have strong reason to believe that it does not depend, as I originally started out with the notion of, on a simple interruption of transmission of nervous force between the medulla oblongata and the liver. The introduction of a sufficient quantity of carbonate of soda (200 grains) into the circulatory system, previous to injuring the sympathetic, checks the production of diabetes. This I consider a most important fact, and I have over and over again verified it, having selected generally the superior cervical ganglia to operate upon. It is true, a diabetic effect does not universally follow removal of these ganglia where no injection of carbonate of soda into the blood has been resorted to; but it generally does so. Where, however, the injection is previously employed, I can confidently look for an absence of any diabetic effect. It has been the same when I have operated on the vertebral filaments ascending from the chest, instead of the superior cervical ganglia.

Although, however, the carbonate of soda thus prevents the appearance of sugar in the urine after lesions of the sympathetic that otherwise occasion it, yet it has no such power where destruction of the medulla oblongata is effected, and the circulation is maintained artificially. The introduction of carbonate of soda into the circulating current is quite ineffectual in preventing the production of sugar in the liver as a post-mortem occurrence, and, therefore, in keeping the urine free from saccharine impregnation when pithing and artificial respiration are performed.—*Lancet, Nov.* 24, *Dec.* 1, 8, 15, 22, *and* 29, 1860, *pp.* 503, 529, 555, 579, 607, 629.

36.—*On Sugar in the Urine.*—"Sugar in the urine," writes M. Henry Mussет, "does not necessarily imply the existence of diabetes. One may pass sugar with the urine, and yet enjoy perfect health. Dr. Blot has shown that sugar exists normally in the urine of all women during parturition, of nurses, and of a certain number of women during pregnancy. Dr. Leudet has shown in paraplegic patients that there is a constant relation between the appearance of

the nervous cerebral accidents and glycosuria. Dr. Itzigsohn relates a remarkable case of traumatic diabetes, occurring in a blacksmith who had received a blow on the top of the head. Dr. Todd has also given a case of diabetes which was observed in a woman after she had received a blow on the head. Prout observed sugar in the urine of dyspeptics and aged persons. And Dr. Goolden in children during dentition. Thus, then, there are numerous circumstances, physiological and pathological, which directly or indirectly concur in the formation of sugar in the economy.

"When we consider that in everybody there are products which are returned by the lymphatics into the general circulation; that the transformation of starch goes on normally in the intestines; and that it is accomplished even in the mouth under the influence of the salivary diastasis; and if, moreover, we recollect—that glycosuria accompanies dentition, dyspepsia, certain cerebral disturbances, that it may be caused by irritation of the brain at the origin of the eighth pair of nerves; that it exists in pregnant and parturient women and nurses—are we not naturally brought to the conclusion, that diabetes is a neurosis troubling the harmony of the assimilating functions?"—*L'Union Méd.—Med. Times and Gazette, Dec.* 10, 1859, *p.* 594.

SURGERY.

AFFECTIONS OF THE BONES AND JOINTS, ETC.

37.—ON AMPUTATION AT THE KNEE-JOINT—AT THE KNEE—AND EXCISION OF THE KNEE-JOINT.

By Richard G. H. Butcher, Esq., Surgeon to Mercer's Hospital, &c., Dublin.

[The ordinary mortality after amputation of the thigh is estimated by Professor Syme at not less than from 50 to 80 per cent. This immense mortality has awakened a spirit of emulation amongst practical surgeons towards perfecting a better method. The circular, the oval, and various modifications of the flap operation have each been in vogue, and temporarily praised as each superior to the other. The cause of the mortality can, in most cases, be traced to the division of the femur, the compact tissue of which is peculiarly dense, and liable to exfoliate, and its lining membrane peculiarly extensive and vascular, and hence liable to inflammation. These dangers are avoided in amputation at the knee-joint, the credit of bringing which operation into notice is due to Professor Syme, though performed so early as 1764, by Hoin. Mr. Fergusson first performed the operation in England. The following case shows the way in which Mr. Butcher amputates just above the knee, and it shows also what may be done by pains and attention in cases of secondary hemorrhage. The whole of Mr. Butcher's paper is so valuable and so full of practical matter that we have felt it difficult to abridge it much without diminishing its value—hence the length of the following extract.]

February 25, 1860. The man being carried into the operating theatre, he was quickly brought under the influence of chloroform; when so, he was drawn down gently to the edge of the operating table, so that his pelvis rested thereon. The right leg was then secured to the leg of the table, so as to guard against any struggles, and permit me free access to the affected limb. The femoral artery was compressed at the groin by an assistant, and the leg steadily supported by another; standing on the right side of the patient, I drew a long, narrow-bladed amputating knife in a curve way across the forepart of the articulation, the heel of the knife being laid on the outer and back surface of the external condyle, at where I thought its cartilage ceased; it was then carried downwards and outwards across the lower margin of the patella, and corresponding to the in-

sertion of its ligament, and then upwards to the internal condyle, at a point opposite to that where the knife was first placed; the flap was then raised off the patella by a few touches of the point of the knife, and then came directly back at its upper edge, severing the rectus and muscles attached to it, and freely opening the joint in front. So well back were the angles of the wound, and so accurately placed, that transfixion was accomplished with facility, the flat of the knife lying evenly against the bone, as it travelled from within outwards; the leg was then very slightly bent, so as to permit the instrument to slide more readily over the posterior projection of the internal condyle; having passed this point, the leg was again straightened, and the knife kept close to the bones throughout the entire extent of the calf, and then made to cut directly out: the anterior flap was next freed from its synovial lining, and lifted from the sides of the condyles, and in front, just free from the margin of the cartilage of incrustation. The flaps being well held up before and behind, and the limb carefully steadied, I laid the fine blade of my own saw on the healthy osseous tissue close to the cartilage, but not infringing upon it, and then cut the bone in a curved manner from before backwards, thus securing even a longer stump, more of the bone free of cartilage, and exempt from sharp and irritating edges, better in every way for adaptation to the soft parts. *I was the first, I believe, to lay stress upon this method of sawing the bones in certain amputations;* thus the limb was severed; the arteries were then secured—seven in all—and the patient conveyed back to bed; he quickly awoke from the effects of the chloroform, which acted most favourably, the patient not having experienced any pain; he bore the operation well, not having lost, I think, a drachm of arterial blood; the flaps were dealt with, and left to glaze; in seven hours I dressed the wound, and it was astonishing how admirably the parts came together; at first it might have been supposed that too much soft parts had been kept, but this proved not to be the case; it will be seen that the same complicated way of forming the posterior flap, as described in the *recent amputation*, was not followed out or necessary here; here the calf was wasted, attenuated from want of exercise, from general emaciation; and here, too, the lower end of the flap was cut straight out, so that, when doubled up, the additional muscular tissue preserved was beautifully covered in by the thin anterior flap; all the surfaces were gently freed from clots; all the surfaces were accurately brought together; the ligatures chiefly being at the posterior angles; several points of the interrupted suture were had recourse to, and some wide straps of adhesive plaster, extending from high up behind the thigh, carried over the face of the stump, and up upon the forepart of the limb, so as to afford a long, gentle traction forwards; the line of juncture secured well in front, and altogether the covering in of the bone was abundant and admirable; pillows were adjusted also to favour this end.

On the 26th, the day after the operation, the pulse had come down from 125 to 100; and in every way the man was greatly benefited by the operation; so quickly did every thing go on, so little pain, so free from discharge, that I only removed one or two of the plasters near where the ligatures lay; so little redness or resentment to the presence of the stitches, that I suffered them to remain until the 8th of March, when I cut them all out, union being effected by first intention throughout the whole track of the wound, and the entire flap well up and fixed in its berth; great caution was employed in giving it due support, both by adhesive straps and pillows nicely arranged.

On the 13th of March the smaller ligatures came away, and on the 16th, the main ligatures (two in number) came away without a trace of blood; this was at 9 a.m.; however, I was sent for at 6 p.m., as bleeding was reported to have come on; the dressings were all stained with scarlet blood, and a few drops occasionally escaped from beneath and through them; on exposing the inner angle, the blood came up quickly from the track through which the ligature was drawn in the morning; I elevated the stump nearly at right angles with the trunk; put a few evenly adjusted compresses over the face of the stump, and made more forcible pressure upon it by wide adhesive straps brought from behind forwards, and then it was propped by pillows; the pad of the compressor used in aneurism was brought to bear gently on the femoral artery, just after it passed off of the pubes; ordered twenty drops of tincture of opium, and fifteen of digitalis, to lower the action of the heart and throbbing vessels; ice was placed in bladders over the dressings, and along the course of the artery in the thigh; thus the bleeding was arrested. This bleeding was to be attributed to the man suddenly sitting up in the bed, and keeping this posture immediately after I left the ward in the morning. No doubt, the sudden change of position excited the violent emotion of the heart's action towards hastening, propelling onwards the current which broke down the recently glued parts, washed away the as yet not perfected plasma. Visited at 9 p.m.; no return of the bleeding; flushing of the face and heat of body gone; he lies in the same position, perfectly horizontal, with the thigh propped up in almost a perpendicular direction; gentle pressure over the femoral artery near the groin, and ice over the dressings; the digitalis and opium to be continued every third hour.

March 17th. Slept quietly; no return whatever of the bleeding; circulation quiet; to continue the opium and digitalis; all drinks cold; milk, a little chicken tea.

18th. No return of bleeding; treatment as on yesterday; parts undisturbed.

20th. No return of the bleeding; so dressed the stump and placed it almost in the horizontal position, slightly raised and supported at the end; the man complaining very much of the fatigue consequent upon the last day's constraint in the raised posture.

On the morning of the 22nd, at six o'clock, I was hastily summoned to see the man, and found him with arterial blood flowing from the same point as it occurred before, but far more rapidly, and in greater quantity. I elevated the limb again at right angles with the trunk, steadied it effectively in this position, put an aneurism clamp with moderate tightness over the femoral artery, high up, and then removed all dressings off the point through which the blood welled up, and put a small dossil of lint in the little hole, a larger over it, and then another, wider and firmer than the former, and upon this steady finger pressure; this I kept up for half an hour myself, and then had relays of students, who unflinchingly gave me their assistance for nine consecutive hours. During all this time the pressure was never suffered to relax; during all this time the same assiduous care characterized their efforts. The digitalis and opium were given internally in somewhat larger doses; and ice-bladders were laid over all the greater portion of the stump, and along the sides and forepart of the limb; to these combined, continuous, and well-devised means, may be ascribed the staunching of the blood, the permanent sealing of the wound, the healing of the part, completed and perfected for ever. After this time no casualty occurred; and in one month after the operation the stump was perfect in every way; at this time I had a photograph taken of the man by a talented young friend; the stump is in every way beautiful in proportions; the flaps which, as before observed, seemed at first too large, now constitute a firm, compact, well-arranged cushion, amply covering the bone and their line of union well in front, and out of the way of pressure or injury. Soon after this, I had an artificial leg made, upon which the stump rested firmly, and with which the man could walk about without the slightest pain or inconvenience; the end of the stump rested firmly on a nice soft cushion, and he did not suffer or complain from jar or pressure. Thus he was going about, when ulceration of a very unhealthy character attacked the cicatrix after the burn on the right leg; it spread rapidly, and assumed a warty character; the entire surface, however, was destroyed, and the part is now just healed, and the poor fellow will be able to get about as well as ever; nothing can be more satisfactory than the state of the stump.

The plan and method of the operation which I have described are, I think, superior for many reasons to amputation at the knee-joint, independent altogether of those cases where the tibia is *extensively diseased*, and where the femur is only in a minor degree affected in its articular surface; but I think it is preferable in all these cases where the leg is so extensively diseased, necrosed, or shattered by violence, that amputation at the knee-joint would be selected, as it is now, by many. I have no doubt whatever that in all cases the cartilaginous surface of the femur should be removed; according to the curved section I have described, cut in this way, the surface for support is just as extensive, wide, and expanded as when the cartilage is untouched;

we find that in many of those cases where it has been retained, unpleasant symptoms have originated from its being attacked by ulceration, and sometimes death. Now, as to the preservation of the condyles to act as a guard to the cicatrix puckered into the hollow behind, a flap being made from the forepart of the limb; even should this security be deemed advisable, it is afforded by my section of the bone being curved a little more; there is no necessity for the risk being incurred of preserving the cartilages, for if the posterior flap be properly formed and brought up nicely in front, the cicatrix will be altogether out of the way of injury; it should not be forgotten, too, that some little difficulty may in certain cases pertain to getting a good covering for the condyles; yet in the hands of a skilful surgeon, this, I think, will not form an objection. The necessity for removing the articular cartilage in amputation at the tibio-tarsal articulation is admitted by all, for the very reason that I urge it in the knee. In the operation at the knee-joint the patella also should be cut out; it is only in the way if retained, and may demand such a procedure at a later period. However admirable a measure amputation at the knee may be considered, it can never take the place of *excision of the joint:* it would then, indeed, cease to be a conservative measure. It is a source of the greatest gratification to me to see how steadily the latter operation is holding its ground, and bearing ample fruit.

Since the publication of my memoirs on excision of the knee-joint, I have been watching with anxious care the results of the operation. The periodicals abound with cases. In the number of the Lancet which has just appeared, while I pen these observations (Aug. 4, 1860), there are nine cases recorded, out of which number one died, a miserable and unpromising case for any operation—"a strumous, delicate man, aged 37." "He had disease of the sternum, co-existing with the ulcerated state of the cartilages of the knee-joint." Evidently the creature was surcharged with scrofula. The remaining cases are reported as most successful in their results, in every instance a limb being secured of seemly proportions, and capable for its several requirements, save flexion. From carefully reading over and maturely considering the several reports of cases published within the last few years, I think I am borne out and confirmed in the several practical deductions which I have arrived at, antecedent to this time, and which I laid down, as some supposed, in rather a peremptory manner; some of those opinions have been copied in my very words, and, I regret to say, without due acknowledgment.

I cannot conclude these observations in a better way, I conceive, than by again briefly condensing those directions which should guide the surgeon when he contemplates excision of the knee-joint, and advert to those truths which should give him a confidence in success.

The first and most important point to be considered is *the judicious selection of the case.* "In order that excision of the knee-joint may prove successful, it becomes imperative that the cases are carefully

selected; by 'successful,' I imply not only the preservation of life, but also the saving of a limb, better than any artificial substitute, no matter how beautifully contrived. It is not applicable, for obvious reasons, where the bones entering into the articulation are very extensively diseased; for though I admit a better chance of preserving life would be secured by excision, even in this case, than by amputation, yet the member would be faulty as a means of progression; it would be short, and a useless appendage. I need scarcely make allusion to organic disease of the viscera as affording insurmountable objection; but this applies, with equal force, to the non-propriety of amputation. In such a condition either operation becomes only justifiable to alleviate excruciating torture. I do not lay much stress upon the integuments being extensively disorganized, or think that such a state militates with any force against excision, for in numerous instances, after removing carious and dead bones, I have been forcibly struck by the remarkable rapidity with which the soft parts set up healthy action and recover themselves; parts undermined and sinuous, when relieved from irritation, and set at rest, become very amenable to simple management. It has been objected to, that after resection the discharge is very great, and runs the patient rapidly into hectic. No doubt, the discharge from the divided surfaces is considerable—may be profuse—yet is healthy; it is essential to the process of reparation, and diminishes, day by day, according to judicious management; it is not so with the ichorous discharge created from the morbid action aroused by the presence of diseased and deadened bones, which, acting as the poisoning supply of the constitutional disturbance, seals the doom of the being unless relieved by operative surgery. It should never be forgotten that resection of the knee-joint is only advocated as a substitute for amputation in certain cases, and not for the simple mode of incising the joint, to which practice Mr. Gay has recently directed the notice of the profession." Again, the same caution upon the necessity of carefully selecting the cases for excision is insisted upon in my second memoir, and published two years later, Feb. 1857. "In my former essay I forcibly dwelt upon the *necessity of carefully selecting the cases for excision*, and pointed out the prominent features which should influence the surgeon; but, I believe the caution has not been applied in every instance. I fear the panting after *éclat* has charmed away some from the stern dictates of judgment; yet I trust this may never be the case—infinite mischief is done by such rashness. Not only is the life of the patient jeopardized by an operation which cannot secure a useful limb for the purposes of life, but the operation itself is brought into disrepute, and open to the sarcastic criticism of those who know but little about it." "The applicability of the operation of excision of the knee-joint to cases of recent accident and gun-shot wounds will depend upon the extent, and be estimated by *the amount of injury the soft parts*, including the vessels and nerves, have sustained, as well as that inflicted upon the bones.

The practical surgeon will be able to compute with accuracy, after careful examination of the parts, how far this method of preservative surgery can be put into practice, and trusted to with hopes of success."

I shall next consider the best mode of operating. In my first memoir I have mentioned the several ways recommended by Park, Mülder, Moreau, MM. Sanson and Begin, Jaeger, Syme, Fergusson, Jones, and Mackenzie, exception alone being taken to that of Syme, and in these words:—"Amongst the many modes of incising the soft parts which I have mentioned there is not one to which exception can fairly be taken except that of Mr. Syme. He advises an elliptical piece to be cut from the anterior wall of the joint, included in the arms of the ellipse, the patella. I do not for a moment doubt the propriety of removing the bone when diseased; but I know no condition that can warrant the cutting away of the flaps; if they are in a properly healthy state, they will not be found too great; after a little time, they will adapt themselves by contraction to the altered state of the parts beneath; if they are perforated by sinuses, and present an appearance which by some may be called disease, they will recover themselves after the carious bones are removed, and not be found too extensive, but still constitute an accurate involucrum for the divided osseous surfaces." The mode of proceeding which I consider best is that described in my first memoir, at page 23, in reference to my first case. "The patient was placed upon a table, lying on his back, and in a few minutes brought under the anæsthetic influence of chloroform. The leg, at extreme of extension, was steadied; the sole of the foot being planted upon the table and held so, forcibly, while the thigh was rigidly fixed by a second assistant. Standing on the left side of the patient, I leant over the knee, steadying its outer side against my chest, and with a strong scalpel cut along the inner side of the joint to about the extent of five inches. This incision was commenced below, at a point about two inches lower than the articulating surface of the tibia, and corresponding to a line a little anterior to its inner sharp edge; the knife was at once thrust down to the bone, and, holding the same relationship, was carried upwards along the femur for three inches; the saphena vein was thus gradually left behind the track of the wound. A similar incision was rapidly made on the outside, commencing below the head of the fibula, and carried upwards above the external condyle; through the entire extent of this, too, the knife was swept along the bones. Both vertical incisions being completed, they were connected with a transverse one, passing an inch above the attachment of the ligamentum patellæ to the tibia; the latter wound opened the joint fully; the lower flap was freed downwards a short way, while that containing the patella was directed upwards, but with some difficulty, owing to the thick, matted cellulo-fibrous tissues which constituted its bulk. The internal and external lateral ligaments were next divided,

together with adventitious bands, the result of organized lymph deposits; the anterior crucial ligament was destroyed, but the deeper fibres of the posterior remained intact, and incorporated by dense structure with the posterior ligament of the joint; these, in turn, were divided, but much difficulty was experienced in detaching them from the posterior surfaces of the bones, with due consideration for the popliteal vessels, which not only lay upon the dense elastic material, but were embedded in it. The knife was then rapidly swept round the condyles of the femur, the disease not extending higher. In the same way the articulating end of the tibia was freed from the soft parts around its circumference; the ligamentous structures being thus cut through, the leg was forcibly flexed, and the ends of the bones thrust forward.

"And now the accuracy of the diagnosis was fully verified. The femur presented its external condyle nearly all removed by caries, while the internal was not at all so extensively diseased, its posterior half being stripped of its cartilage of incrustation, and carious behind the intervening space; between the condyles was likewise carious: the external condyle of the tibia had its cartilages of incrustation removed, which was replaced by a thick fibrous substitute, while the internal presented its normal appearance (we had here strong evidence of the efforts of nature to check disease); the patella was quite carious, hollowed, and reduced to a complete shell; therefore it was dissected carefully out, the integuments in front and covering it being preserved together with the fibrous attachments implanted at its upper and lower edges. The bones being sufficiently exposed, I next proceeded to cut off their extremities, and for this purpose used the saw, which I prefer for amputations, its blade being turned in the supports and steadied so, it was easily passed behind the condyles of the femur, and made to cut forwards; a few movements were sufficient to complete the section. In a similar way its serrated edge was placed behind the tibia, and urged forwards so as to remove a thin osseous slice, together with the surface of the bone. The head of the fibula was not diseased; lying below the surface of the tibia, it did not prevent the apposition of the bones; it was therefore left intact for this special reason, as well as that the attachment of the biceps should not be interfered with. The entire amount of bone removed measured two inches."

There are some points more minutely dwelt upon in the *operation* in *my second successful case*, published in my Reports in Operative Surgery, and which I shall quote. The operation was performed April 15, 1857, on a man aged twenty-seven years.—"The patient being placed under the influence of chloroform, I adopted the H incision, the cross line passing beneath the patella; the flaps were with rapidity dissected back, and the shreds of the crucial ligament spared by disease were divided, and next the lateral ligaments; in freeing the ligamentous attachments to the bones behind, the greatest precaution was adopted; all being separated to the extent required, I swept the knife

around the tibia and the femur, close to the attachment of the soft parts, and then took the saw bearing my name, and cut the bones from behind forwards."

"It is necessary here to lay caution on the operator in using the saw. He should ever remember the altered position of the limb, to facilitate protrusion of the ends of the bones, and, according to the angle of elevation must the direction of the blade of the saw traverse. The simple rule I would lay down for the correct execution of the section is this:—The blade of the saw must pass in a direction parallel with a line drawn in the transverse axis of the articulating surface; accordingly, this procedure was carried out. Thus, when the limb is placed in a horizontal position, the one in which it is to be maintained for cure, the cut surfaces of the bones will lie evenly together, no space will intervene between them behind or before; the wide surfaces oppose each other; all disposition to gliding one from the other is guarded against, and the most favourable circumstances are insured for intimate union. In the published record of cases it will appear that in some instances the surgeon has had to apply the saw a second and third time, to make the bones meet: if this be so, I am warranted in enforcing my advice.

"By section planned after this method the condyles of the femur, with their connecting osseous bond, to the depth of a quarter of an inch in thickness, were cut off. To warrant the removal of these parts, I may just state, the incrustating cartilages of the condyles were entirely removed; the head of the tibia was similarly affected, and, in addition, deep pits were excavated by caries in each condyle, to the depth of a quarter of an inch. This being effected, all the thickened and diseased synovial membrane was clipped away, and the disorganized fatty mass below the patella; not a trace of the interarticular cartilages remained; the patella was coated with lymph beneath, and appeared to have struggled healthily from the disease around; it was therefore suffered to remain. Thus, then, the accuracy of the diagnosis was established, and examination of the osseous surfaces pronounced them healthy. Three arteries which bled rather freely were next tied; the flaps at the transverse incision were brought together, and maintained so by five points of interrupted suture; and the lateral incisions were left open for the ready escape of blood and serum, the purging of the cut parts. The leg was with ease put into the straight position, and placed at once in the padded box splint I had prepared for its reception. A splint was then laid over the interior part of the thigh, and the tapes fastened, sustaining upwards the hinged sides of the box; the foot was steadied by a foot-board, falling into the grooves within, and thus the leg was pressed upwards so as to keep the divided osseous surfaces in contact; lint steeped in cold water was laid along the lateral incisions, and maintained accurately in position by the sides of the box when elevated."

In my *third successful case of excision of the knee-joint* will be found

some further precautions which should be borne in mind by the surgeon about to perform the operation.

I shall transcribe the case. Patient, aged 15 :—

"On the 11th of January, 1858, I excised the joint, adopting the H incision; quickly the soft parts were divided and reflected, and cautiously they were freed from the bones behind. The ligamentous structures within the joint, the cartilages of incrustation of the three bones were entirely removed, and the contiguous surfaces of the bones most extensively destroyed. The condyles of the femur were hollowed out and eaten away to about an inch in extent, while the surface of the tibia was also deeply excavated, corresponding to each condyle, the intervening part being irregularly removed, the patella was also deeply carious; hence I removed it at once from the upper flap. The diseased extremity of the femur was cut with the saw invented by myself, and the section made from behind forwards; and so likewise the unhealthy surface of the tibia. The section of the femur revealed a beautifully healthy aspect, while the section of the tibia showed a softened irregular patch, about the size of a shilling, with a vascular fringe running round its confines, and separating it from the healthy bone outside. This appearance occasioned me to cut off another slice from the tibia somewhat more than a quarter of an inch in thickness, and the section exposed a perfectly healthy condition, the normal arrangement of the bone. In all, there were about three-quarters of an inch, or a little more, of the tibia removed, and an inch and a half of the femur. The divided bloodvessels were small; no ligatures were required; and I at once placed the limb in the extended position, resting upon its posterior surface in the apparatus which bears my name. Let it be borne in mind that, previous to the operation, the leg was considerably flexed and the hamstrings rigidly contracted. This condition offered serious opposition to the required posture at first, but by gentle, steady, and gradually increased force, they were compelled to yield. Now, in the adjustment of the bones, there is one practical point which I have not before alluded to, *neither is it noticed, so far as I am aware, by any writer on this subject*, namely, the caution that is requisite, when straightening the limb, *to guard against any portion of the soft parts from behind projecting forwards, and thus interposing between* the bones. Such an occurrence took place in this instance during my efforts to get the limb into a horizontal posture, but I perceived and rectified it at once; however, it might be easily overlooked, and, I have no doubt, would interfere in a very material manner with the firm union of the bones, or their growth into each other—a consummation so ardently sought for in these cases. Now, this untoward circumstance may be guarded against by the following manœuvre; namely, to adjust accurately, while the leg is yet flexed, the posterior margin of the cut surfaces of the tibia and femur, and while the sharp edge of one bone rests upon that of the other, the tibia and femur are gently forced backwards until the opposed surfaces rest fairly against

each other. During this manipulation, then, the bones are pressed up firmly against each other, as well as with an equal force backwards. By this precaution the interposition of any softened structure will be effectually prevented. After this manner, then, the bones were fairly applied to each other, and afterwards the flaps laid down. They were brought together throughout the transverse incision by a few points of the interrupted suture, while some folds of lint were laid in the lateral ones, the object being twofold: first, to prevent any immediate flow of blood, and, secondly, at a later period, to permit a free escape from the part, so that purulent matter should not be pent up; the front splint was laid down, and each side of the box was then lifted, and an additional compress of lint placed over the wound at either side, so that an even, equable support was given, and maintained. During this entire adjustment, from first to last, the leg was pressed up against the thigh; and, to secure it in this position, the foot-board slid into the grooves for its reception was most efficient, steadying also the foot at a right angle with the leg. An additional pad, wedge-shaped, was placed at the outer ankle, and projecting upwards, so as to maintain the foot in a straight line, and prevent any drooping outwards or eversion. The straps were then buckled round the box, and the patient, still under the influence of chloroform, removed carefully to bed. She, however, awoke from her sleep quite unaware that the operation had been performed."

The next question to be considered is, *what should be the mode of managing the limb after the operation*? I have answered this in each of the foregoing cases. *It should be at once forced into the horizontal position, and put up before the patient is removed from the operating table.* I claim priority for this advice and most valuable maxim, because the very words I have written have been copied, and without acknowledgment. In my first memoir, 1855, p. 58, it is written: "Immediately after excision of the joint is accomplished, *before the patient is taken from the operating table, the limb should be placed in the extended position, and retained so immovably in a solid case*, such as I have described. In some instances it has been shown that the difficulty to force back the femur, and prevent its projecting in front, has been very great; by proper manipulation, this difficulty can be overcome without violence, by one or other of the following measures. Generally speaking, this, the milder method, will succeed; when the end of the bones are cut away, gradual yet powerful extension, continued for ten or fifteen minutes, and longer if necessary, should be made upon the leg, so as to counteract, tire out, and subdue the violent contraction of the hamstring muscles. During this manœuvre the thigh should be gently, yet steadily, pressed backwards, so that the cut surfaces of the bones may be opposed to each other, and then, being pressed back into a straight line, and retained so by proper supports, they offer mechanical resistance to each other, and thus displacement is prevented; but if this method fails, and it will most

likely do so in those cases where the leg has been for a length of time flexed upon the thigh, and the muscles have assumed a spastic contraction of a settled character—here I would most certainly recommend the surgeon to *divide the hamstring tendons in preference to cutting off another piece of the healthy bone.* This becomes more imperative when the head of the fibula has not been removed, and the tendon of the biceps interfered with; it is the powerful and spasmodic action of these muscles, dragging the leg upwards and backwards, that creates to a great extent the deformity, by the thrusting of the thigh bone forwards; *by their division, then,* not only is reduction easily secured, but all tendency to after-displacement checked. *The limb,* I repeat it, *should be secured from the very first in the extended position,* and rigidly maintained so through the entire cure. In my mind the same arguments apply here as in the treatment of fractures of the thigh—the object of the surgeon being in either case to avert spasm, and to obtain, as quickly as possible, a permanent osseous union between the disunited bones." And again, in my second memoir, the practice is as forcibly inculcated at page 62: "I do not believe one word of the 'impossibility' of placing the limb in the straight position at once, and retaining it so; it can be done if the surgeon is up to his work; and this first adjustment can be made without any pain to the patient, as chloroform annihilates sensibility. I cannot find words to enforce, with the power I desire, the importance of this measure. Amongst the foregoing cases there are lamentable instances where it was not adhered to, and which, I trust, will appeal forcibly to the mind of every thinking man. Independent of the advantages of steadying the cut surfaces, the prevention of the divided bones from irritating the surrounding tender parts, the subjugation of all spasm, the limiting of the inflammation to the bounds only necessary for repair, —we have still another great benefit accruing from *putting up the limb at once,* namely, the mind of the patient is at rest; that protective watchfulness over it is removed, and which probably, prior to the operation, had caused nights of restlessness and want of sleep. The same apprehensiveness, when the bones are divided, will remain, aye, be increased, if the limb be not immediately fixed in the straight position; but if the control of the patient over the part be checked, he feels confident in the security; repose quickly follows, and sleep is generally induced; if not, opiates will act more certainly, pain being subdued."

I am confident that the box-splint which I have used in all my cases is by far the best apparatus for securing immobility of the divided parts in a proper axis, preventing projection forwards of the thigh-bone, keeping the divided osseous surfaces in contact, permitting a safe and ready way of dressing the limb without disturbance, and affording, at the same time, the greatest amount of ease and comfort to the patient under existing circumstances. The apparatus is figured

as applied to one of my cases, in my Reports on Operative Surgery, and a description is thus given in my first memoir, two years before, at page 25: "The limb was placed in a horizontal position in a wooden case which I had made for the purpose. The sides were attached to the back part by hinges, so as to allow of being let down at the time of dressing; they were likewise of unequal lengths, the internal extending nearly as high as the ramus of the pubes, while the external passed up to the axilla, similar to the long splint used by me in fractures of the thigh; the lower end of each lateral piece presented on the inner surface a number of grooves about an inch apart, so that, when the sides were elevated, the foot-board was received into any opposite pair of them, according to the distance required; this lower piece acted in two ways; not only did it maintain the foot at a right angle with the leg, but it steadied the sides, and prevented their being pressed inwards from their vertical direction by the tapes and buckles which girthed the apparatus on the outside. The box was supplied with hair-cushions, carefully adapted to its entire extent, some being covered with oiled silk. In addition to the posterior, lateral, and foot support, a broad splint, well padded, had to be placed over the anterior surface of the thigh, extending from a little below Poupart's ligament as far as the junction of the upper and middle thirds of the leg, and secured firmly down by the surrounding web-belts, so as to counteract the powerful tendency towards the distortion of the limb forwards. I have already mentioned that the external side of the case passed up to the axilla, the object being to secure the straight position for the limb. It was kept in contact with the trunk by a wide girth passed around both. The patient expressed himself as being very comfortable with the limb done up in this way, and it was satisfactory to the surgeon to behold it, every requirement seemed so entirely fulfilled."

Here, again, I wish to claim *priority* for *the application of the anterior splint and likewise for the extension of the external one along the trunk, as also for the foot-board*, gently retaining the foot in a proper axis, and the tibia in contact with the femur; *it is a great error to suppose that this arrangement is not most advantageous, just as great as to suppose that the fractured extremities of a broken bone should not be placed in contact, or to argue that, because adapted, ulceration would be likely to follow.*

The more rigidly the limb can be united the better, for the reasons expressed at page 59, Memoir I.: "Some operators contend that a slight degree of flexion and extension is desirable after excision of the knee-joint. To this opinion I cannot subscribe for two reasons: first, because as a means of sustentation the limb would be found inadequate; and not so sightly in appearance; secondly, the very motion perpetuated between these bones, already prone to carry on unhealthy action, would become an exciting cause for a renewed development of disease. No; I conceive, for perfection as a result of

excision, *the tibia and femur should be grown into each other and bound by callus*: and, far better still, when the patella, deprived of its cartilage, and undisturbed from its berth in front, becomes fused into this connexion. A limb cured in this way will harmlessly endure the fatigues of travel. An objection has been made to uniting the limb in one solid piece, on the grounds of its liability to fracture being far greater than when some motion is permitted between the bones. My answer is, such a result has never taken place; and I am equally certain that the force required to break the callus union or limb in any part of its length would entail far more grave and serious mischief if applied to a limb enjoying partial motion; in the latter, the fibro-ligamentous structures would be all torn up from their connexions, and, as a sequence, high and active inflammation readily set up, formed by the rapid formation of matter, and all its train of serious consequences. Even suppose this burst of mischief checked by energetic treatment, the danger would not be removed; a slow, insidious action would rouse up the latent disease, which in turn would prey upon the general health, until the only chance of preserving life would centre in amputation. Now, in the former case, no such dismal consequences present themselves; the fractured limb might be brought to its full length, and maintained so by the application of the long splint, until union be again accomplished." I have laid great stress upon *the careful dressing of the wound, and the free administration of sedatives, stimulants, and support at suitable times in these cases*, and most strongly insisted upon the practice. "The amount of success following all severe operative measures greatly depends upon the care and assiduity bestowed on the after-treatment, both locally and constitutionally. Probably, in the entire range of operative surgery, there is no class of cases which demands such close and attentive looking-after as that now under consideration. It will not be sufficient for the surgeon after he completes a severe resection, be it in ever so masterly a manner, to delegate his duties to another, and, after he puts aside the knife to consider his part done. No; if he is desirous of success, he must use his own hands, and be the dresser, while by his watchful eye he will readily discover the early threatening of incipient mischief. After all severe surgical operations I am in the habit of supplying stimulants and sedatives very freely, together with nutritious food apportioned to the assimilating powers of the individual. This treatment is more imperatively urgent when the patient is advanced in life, and has endured lingering disease for a length of time. It is, I would say, equally necessary to the infant and the child; whereas, in reference to the middle periods of life, the diminution or increase of supply must to a great extent rest upon the judgment of the practitioner. By the copious administration of stimulants the flagging powers of life are upheld; by the free exhibition of sedatives, pain and irritation and spasm are subdued." Relative to the shock of the operation and the adminis-

tration of chloroform, I have thus written:—"*Happily, the shock now need not be so greatly estimated or dwelt upon.* Chloroform protects the sufferer; and I certainly attribute much of the success which in latter days has attended this formidable operation, to the beneficial agency of this potent medicine. In nearly all the cases which I have collected, it has been used; in every successful instance narrated it has been given. In the case upon which I operated, the man was reduced to the lowest state, and would almost to a certainty have sunk from the shock either after amputation or excision; yet by this powerful means all violent struggles were prevented, and, of course, the patient saved from subsequent exhaustion. How different this tranquil sleep from the writhing torture of the sufferer under a similar operation described by Sir Philip Crampton: "The poor girl, who, in coming into the operating room, exhibited the greatest fortitude, and even cheerfulness, on the instant that the knife was applied to the skin became so ungovernable that four strong assistants could with the utmost difficulty retain her upon the table. This necessarily prolonged the operation, and no doubt very much increased its severity; the removal of the divided extremity of the femur was here rendered a work of infinite difficulty and danger, as, when the knife was passing between the popliteal artery and the bone, and actually in contact with the former, no entreaty could induce the poor girl, *whom terror seemed to have deprived of her reason*, to remain for one moment at rest, she struggled so violently with both limbs." *In every case of resection which I have performed I have used chloroform, and to its administration I again repeat is due, I think, in a great measure the invariable success which I have met with.*

Briefly to recapitulate those directions for excision of the knee-joint, which I have laid down, and would still insist upon:—

1. *The judicious selection of the case.*—The bones not being diseased far beyond their articular surfaces; while, if upon section found to be a little more than had been expected, the part should be gouged out, or an additional thin slice removed; but, if to a greater extent, amputation should be at once resorted to, and, as recorded in my first memoir, with a hope of excellent success. Again, the Report goes on to show that amputation may be performed some days after excision, should any unfortunate circumstance in the management of the case have arisen to demand it. In this same paper seven instances are recorded of amputation of the thigh, and all made rapid recovery, save one.

2. *The H incision should be preferred.*—The perpendicular strokes placed well back, so as to allow all fluids and discharges to drain off—far more effective and safer than any opening made in the popliteal space.

No portion of the flaps to be curtailed, though they may be thinned, of any thickened fibrinous matter or diseased synovial membrane; the latter, particularly, should be clipped away with a strong scissors. All

ligamentous fibres, both around and within the joint, should be cut through, and the extremities of the bones fairly freed and exposed.

3. *The patella should be taken away in all cases, whether deceased or not*, and then the section of the bones, well thrust out in front, should be made with "Butcher's saw" from behind forward, due attention being paid to the axis of the thigh bone at the time of its division.

4. *All bleeding vessels should be tied, or any that have sprung and retracted should be drawn out and secured*, so as to guard against intermediary hemorrhage.

5. *While the patient is yet on the operating table, the limb should be placed in the horizontal position, either by gentle and steady traction, combined with pressure of the cut surfaces of the bones backwards, or, if necessary, the division of the hamstring tendons.* Their support behind, in every case, I look upon as of great value, therefore their section must be looked upon as a last expedient towards straightening the limb.

6. *During the adjustment of the bones, great caution should be exercised, that their surfaces be throughout their extent in contact, and that no soft parts intervene.* The flaps should be then laid down, and connected by suture closely throughout their transverse division, while the lateral incisions should be brought together only at their extremities by one or two points, and the central portion of each, that corresponding to the division of the bones, should not be brought in contact, but dressed lightly with lint soaked in oil, thus securing a ready outlet for the escape of fluids. The extremity should next be cautiously laid upon "Butcher's box splint," padded to the natural configuration of the limb, its sides elevated, foot-board applied, suitable pads introduced, and then the anterior splint laid on, taking the place of the assistant's hand, which, from the first, restrained the femur from projecting forward; then the straps buckled, the waist-band applied, and the patient may, with safety, be removed to his bed. The bed should be prepared in this way, and consist of a couple of hair mattresses laid one upon the other, evenly supported, and intervening between the upper one and the sheet, a folded blanket, feather pillows for supporting the head and shoulders; the bed should be, likewise, moderately warmed, so as to prevent the patient being chilled when put into it.

7. *The limb should not be disturbed for several days;* the length of time depending a good deal on the season of the year when the operation is performed, whether it be in the heat of summer or the cold of winter. After five or six days it may be necessary to let down the sides of the box-splint, to sop up discharge, change lateral pads, and soiled dressings, &c. By the apparatus named, the facilities for cleansing the limb are so efficient, that it may not be requisite to lif the member from its support for even so long a period as three weeks, as evidenced in my own practice. Should, however, it be con-

sidered expedient to change all the dressings, the anterior splint should be steadily held back by an assistant, and the limb pressed up to it, thus guarding against any starting of the femur forwards, or displacement laterally, when lifted from its bed. When the box is prepared, freshly arranged, the limb, controlled after the manner mentioned, should be laid down, the side splints elevated, foot-board secured, and the straps over the anterior splint first tightened so as to maintain it in that position, from which it was never suffered to change. I would impress this advice still further—if the straps be unloosed for any purpose, *the hand of an assistant should steadily keep the anterior splint in its position, and well pressed back, until the artificial support is again brought to bear upon it, and fastened.*

8. *In cases were large abscesses form in the vicinity of the excised joint, or up along the thigh, Chassaignac's drainage tubes may be used with the best hopes of success.*

9. *The free administration of stimulants and sedatives, imperatively demanded in all cases of excision*, regulated to a certain extent. by age, sex, temperament, and habits.—*Dublin Quarterly Journal, Nov.* 1860, *p.* 267.

38.—PROGNOSIS AND TREATMENT OF SEVERAL VARIETIES OF CHRONIC DISEASES OF THE JOINTS.

By WILLIAM PIRRIE, Esq., Professor of Surgery in the University of Aberdeen.

[We take the following extract from the 2nd edition of the author's work on "The Principles and Practice of Surgery." The work itself is practical and the descriptions are concise and clear.]

I have been at great pains, and have spent a considerable sum of money, for the purpose of arriving at a correct conclusion as to the curability of this disease of the joints (scrofulous chronic synovitis), and the firm belief at which I have arrived is, that in the event of the disease not being so far advanced as to have given rise to disorganization of the joint, almost every case is perfectly curable, on the expenditure of a sufficient amount of money in procuring the use of proper remedies. I have taken a great interest in this class of cases for many years, and have been long in the habit of treating some at my own expense among the poor, and have not a stronger belief in almost any surgical subject than in this, that unless total or very extensive disorganization of the joint has taken place, most cases, however unpromising, are curable. The treatment from the use of which I have seen such gratifying results consists in residence in the country, in a dry, bracing atmosphere, with much exposure in the open air to the light of the sun; in maintaining the atmosphere in the patient's room perfectly pure by day and by night; in sleeping in a room in which there is free admission of the light of the sun during

a reasonable portion of each day; in guarding against anything which could cause any impurity of the atmosphere; in sleeping in a bed so arranged as to allow free circulation of air around the patient, and as much exposure to the light as possible; in the daily use of animal food, cream, and cod-liver oil, along with some of the preparations of iron; in maintaining a healthy condition of the skin, by the means proper for that purpose; in watching the condition of the digestive organs, and maintaining them in a vigorous state; and, if necessary, giving some alkaline preparation to correct the acidity of stomach, which is so injurious in the scrofulous diathesis, and so certain, in the way explained in the chapter on tubercle, to keep up the error in the constitution of the blood, which constitutes so great a part of the scrofulous diathesis. These means, along with cheerfulness, mental occupation, encouragement, and suitable local treatment, certainly lead in most unpromising cases, with very few exceptions, to the best results that can be desired. It is exceedingly important to encourage the patient, and to produce a firm conviction that benefits will result from treatment. I was much struck with the manner in which this was expressed to me by a patient some years ago. The patient was a female, who had a bad knee; she had made up her mind to allow it to be removed, and as it was a very unpromising case I thought it an excellent one for testing the usefulness of the above mentioned treatment. I got the treatment instituted to the utmost extent I could desire; the knee got perfectly well, and when the woman, who I have no doubt was a sincere Christian, called on me to thank me for the great interest I had taken in her case, she remarked:—'I find medicine, like the Gospel, must be received in perfect faith to get the full benefit of it; I had such comfort and pleasure in following all your directions, even at my worst, because I felt sure they would do me good, as you were always confident that I should get better." The reason why so many poor people lose their limbs from this disease is, that their poverty renders them unable to place themselves in circumstances to obtain the remedies essential for cure. I have often thought it a matter of the deepest regret that persons who leave much for benevolent purposes, seem not aware how beneficially funds might be appropriated if left for maintaining and treating in the country, in healthy situations, hundreds of the poor of our towns, who are constantly dying or losing limbs from the effects of scrofula.—*British and Foreign Med.-Chir. Review, Jan.* 1861, *p.* 127.

39.—ON THE TREATMENT OF CONTRACTIONS OF THE KNEE-JOINT.

By Holmes Coote, Esq., Assistant-Surgeon to St. Bartholomew's and to the Royal Orthopœdic Hospital.

[The usual inflammatory affections of the knee-joint, whether acute or chronic, strumous, gouty, or rheumatic, terminate naturally in by

by a greater number of cases than it is commonly thought. The patient is left with the utility of the limb lessened, but not destroyed. There are, however, various complications, especially extension of inflammation to the cancellous head of the bone, from imprudent use of the limb, which prevents the disease coming to its natural termination.]

In the present lecture, one great object which I have in view is to urge you, even in the severer cases, to be in no hurry to operate, whether your inclinations may lead you to amputation or excision. Study rather the processes of repair which nature can effect, and then see how, by mechanical after-treatment, you may rectify either weakness of the proper structures or faults of position.

I have a great objection to quote general statistics, because they include patients of every age, and diseases of infinite variety; but I believe that the favourable returns presented by those who have written of late upon both these operations proceed from the fact, that many of the patients have been young subjects; indeed a considerable proportion have been infants.

Now I protest against the practice of excising a knee-joint, except in very exceptional cases, in children. Even the practice of amputating the thigh in such cases is nearly out of date, and you do not often witness either of the operations here. Out of 46 cases of amputation of the thigh, the total number performed in St. Bartholomew's Hospital since January, 1857, to the present January, 1861,—a period of four years,—seven only of the patients have been under fifteen years of age, and out of that number, three of the operations were undertaken on account of severe accidents, leaving only four operations on account of diseased joints, or about one a year. And of those four children, two were in a state of extreme emaciation; a third was suffering from hæmoptysis.

Upon mentioning these statistics to a friend of considerable experience, I was told that the explanation of the rarity of operations for diseases of the knee at this hospital proceeded from the fact, that the severe cases were no longer sent to us; for surgeons now-a-days undertook their management elsewhere. I endeavoured to disabuse his mind of the idea, but I fear, without avail. But, perhaps, I may be more successful with you. The reason is to be found in the greater patience on the surgeon's part; in the application of sounder principles of treatment. The cases are of the same character as formerly, but somehow patients themselves have made the discovery, that time works wonders in matters of restoration and repair. The records of the Orthopœdic Hospital will show case after case in which patients have persisted in retaining limbs condemned three or four times. They present themselves at that institution after all morbid action has ceased, with a limb contracted, and perhaps, shrunken, but yet admitting of replacement in the extended position. The rarity of

amputation of the thigh for the removal of a diseased knee in the upper ranks of life has long been a common remark amongst surgeons.

In the last number of *Braithwaite's Retrospect*, (vol. xlii., p. 168,) there are given the particulars of nine cases of resection of the knee-joint, performed in different hospitals, (originally reported in the Lancet,) and we there learn, that 6 out of the number were under 18 years of age; indeed, 4 were between the ages of 12 and 6. All these children recovered from the operation, although in some cases the after-symptoms were serious. The remaining 3 patients were respectively of the ages of 23, 33, and 37. They recovered from the shock of the operation but one died subsequently. In order to give a fair statistical return of the results of these operations, we should put them into two classes—the young (in whom I deny the propriety of operating) and the middle-aged (in whom, *under some circumstances*, the question may, perhaps, be raised); and we then see that in the latter, the mortality, from the above report is 1 in 3. But these data alone are insufficient for any useful conclusion, though not very far from the truth. Mr. Butcher and Mr. Price, avowed advocates for excision, give, after a general summary of cases, the fatality (as applied to the knee) as 1 in 5, and this, as Mr. Bryant remarks, shows the most favourable aspect.

But we must not confine our remarks to the immediate results of the operation. It is no argument in its favour to say that we kill only 1 in 5, or 1 in 3. It is incumbent on its advocates to show that the patient derives some substantial benefit in return for the risk which he has run; that the limb which remains to him is a good and useful one. Allow me to direct your attention to this diagram. It illustrates one of the results following excision of the knee performed in infancy—namely, arrest of development of the entire limb.

The particulars of the case have been published by Mr. Oliver Pemberton, surgeon to the Birmingham General Hospital. E. F., aged twelve, a pale, strumous-looking boy, of much intelligence, was admitted Dec. 20th, 1853, into the General Hospital, Birmingham, suffering from disease of the left knee of fourteen months' duration. The leg was bent at right angles; the circumference of the joint larger by three inches and a half than that of the opposite one, the integuments were shiny and painful; the apertures of three or four sinuses were apparent, and three communicated with carious bone. His sufferings were very great. Mr. Pemberton excised the joint Feb. 8th, 1854. The amount of bone removed measured rather more than three inches and a half. About two inches and a half belonged to the femur, and one inch to the tibia. The fibula was untouched. The patella was left, its under surface having been scraped. The case went on favourably. When discharged eight months subsequently to the operation, the wound had entirely healed. He could walk, with the aid of a stick and a high-heeled shoe, the knee being supported by a

leather case. "After the lapse of six years," continues Mr. Pemberton, "I place on record the following condition of this patient's limb:—He had grown in height, but was still diminutive for his age. The lower limbs presented a wonderful contrast in appearance. The one was strong, with the muscles, bones, and joints well defined; the other, feeble and blighted. The sound limb, from the anterior superior spinous process of the ilium to the outer malleolus, measured thirty-four inches; the one subjected to operation, twenty-five!—thus showing a difference of nine inches. The femur, tibia, and fibula of the limb were not larger than the same bones in a child of ten or twelve."

In confirmation of this important fact, I may quote the authority of Mr. Syme. "I tried the operation," he remarks, "nearly twenty years ago on a boy, who perfectly recovered from it, and seemed at first to possess a limb little inferior to its fellow, except in so far as it was stiff at the knee. But in the course of time it was found that the growth of the two limbs was not equal, and that the one which had been the subject of operation gradually diminished in respective length, until it wanted several inches of reaching the ground when the patient stood erect."

In 1858 I saw a similar case of withering of the limb after excision, in a boy in whose history I could not understand the necessity of this operation, and in whom a favourable issue was reported in some of the journals.

When inflammatory disease of a joint, whether acute or chronic, has subsided, it often leaves the limb in a state of anchylosis—that is to say, the leg is bent on the thigh at an inclination more or less marked; the tibia and fibula are retracted and generally rotated outwards, so that the condyles of the femur, and especially the internal, present an under projection.

It is in this condition of the limb that we are called upon to make use of orthopœdic appliances, and it is therefore desirable to ascertain, as far as possible, the exact condition of the morbid parts and the character of the resistance.

In the first place, true bony anchylosis—that is to say, a firm osseous blending of two separate bones, is of uncommon occurrence, especially in the knee. The late Mr. Langstaff used to say that it never occurs after scrofolous disease, and I believe that his opinion is correct. I once heard a distinguished surgeon affirm that preparations of osseous anchylosis were to be found hanging "by the dozen" in every museum. That statement is quite at variance with my investigations both here and elsewhere. True osseous anchylosis, I repeat, is very rare, especially in the knee, and is in general one of the consequences after acute rheumatic arthritis. In the museum of this hospital (Ser. II., Nos. 29, 48, 55) there are a few specimens of firm bony anchylosis. In Series II. we find two of the knee and one of the hip. In the knee-joint the patella is in one case (No. 55) united by

bone to the outer condyle; in the other case (No. 29) the patella is also united to the outer condyle, and "the cancellous tissues have coalesced." In Sub-series B, consisting of dried preparations, there are but nineteen specimens—namely, five of the hip, four of the knee (in which, again, union of the patella to the outer condyle is the prominent change), three of the sacrum and ilium, and one of the shoulder, three of the elbow, two of the carpus, and one of the finger. But these are not all cases of firm bony union. In the greater number only a light spongy osseous material, easily broken down, is thrown out, as in the specimen now before you. You find, in general, anchylosis to depend on displacement of bones combined with the formation and organization of ligamentous bands. In one specimen (Ser. II., No. 7) the opposed surfaces of the synovial membrane have become united by the organization of lymph. In other cases (No. 13) anchylosis has taken place both by adhesion of surfaces and consolidation of surrounding parts. In a third class (No. 29) the union is by false membrane and by friable bone. It is important to bear these several points in mind, that you may decide how and with what means the resistance is to be overcome.

Before proceeding to put a contracted limb straight, you must be certain that all morbid action has ceased. There must be neither heat of skin, swelling, nor pain; the disease must have run its course, during which it generally produces degeneration of the more highly organized tissues. You next ascertain the history of the case, and bear in mind that the connecting bands of adhesion are in all probability fibrous. If you doubt, make gentle extension with one hand, while you feel the flexor tendons in the ham with the other. If these tendons start up at each attempt to move the limb, you may be sure that there is some amount of movement, whatever be the apparent rigidity, and that consequently the union cannot be osseous.

Remember also, in reference to treatment, that in nearly all these cases the tissues peculiar to a joint have been destroyed or injured. You can do, therefore, little more than overcome contractions, and place bones in more favourable positions. The power of restoring motion is limited to a small class of cases—namely, those in which the loss of motion depends on thickening *external* to the joint, the cartilage and synovial membrane being in a normal state. I have little personal experience in such cases; but several have been reported by my colleague, Mr. Brodhurst, whose work "On Anchylosis" you may peruse with interest. He affirms that, in certain forms of disease, such as rheumatic synovitis, gonorrhœal rheumatism, &c., "the inflammatory product is poured out into the cellular tissue around the joint, giving rise to induration, with thickening and consolidation of the soft structures into and about which it is effused. It is not uncommon to find that the cellular and fibrous tissues around the articulation alone have suffered from the inflammatory deposit, the synovial membrane remaining unaffected and clear, and the interior of the joint free

from all effects of inflammation." Acting upon these conclusions, he is in the habit in such cases of forcibly flexing and extending the joint, the patient being under the anæsthetic influence of chloroform, and of endeavouring to preserve such mobility by subsequent passive exercise. He relates many cases; but the results of my own experience are that the interior of the joint suffers far more frequently than he supposes. There are cases in which a healthy joint may be contracted by cicatrices external to the articulation: here free motion may be restored. Or a joint may become stiff from external thickening; but, in general, such thickening disappears as the morbid action ceases. I have often had recourse to forcible extension: but then only to break down some strong band of adhesion, after which I prefer to make extension gently. Motion cannot be restored when the articular surfaces have been injured.—*Lancet, March* 23, 1861 *p.* 283.

40.—ON CONTRACTIONS AT THE SHOULDER-JOINT.

By HOLMES COOTE, Esq., Assistant-Surgeon to St. Bartholomew's and to the Royal Orthopœdic Hospital.

[The merits of excision of the joints of the upper extremity have been over-rated, and too hastily named *conservative*. They are often performed when it would have been better to abstain from surgical interference altogether. Diseases of the joints of the upper extremity are less dangerous to life, than when the lower extremity is the seat of the affection; for the patient is not of necessity confined to bed, and may avail himself of the advantages of change of air and scene.]

If you ask me what are those cases in which the operation of excision is indicated, I must reply by beginning with a process of exclusion. Certainly not in the case of young subjects, nor as a rule even amongst adults when suffering from strumous disease, or under any circumstances where the disease is recent and active, nor where the patient does not suffer much pain. There is nothing to be proud of in cutting out a joint. To use an old expression, "we mangle that which we profess to be unable to cure;" and, believe me, the proceeding is not wholly free from danger. In July, 1857, a man about forty years of age came under my care in this hospital suffering from rheumatic disease of the right shoulder-joint. About that time the surgical mind was a little upset by the glowing accounts which prevailed of the benefits which ensued from the removal of diseased articular surfaces, and I confess that mine was unduly influenced also. This patient had been a sufferer for many months; abscesses had formed round the part and burst, leaving sinuses which led down to denuded bone; the slightest movement of the joint was productive of pain. Well, I excised the head of the bone. The operation was performed in the usual way and without difficulty, and I anticipated a favourable issue; but abscesses formed along the limb, and the man died in the course

of a few weeks of hectic. Upon examining the head of the bone after removal, I felt convinced that there were no morbid changes irreparable in character; and surely this man would have had a better chance had I left the diseased parts *in situ* and trusted to rest and soothing treatment.

The operation of resection may be undertaken with advantage in cases of inflammatory disease of the shoulder-joint, when the local ailment seems likely to wear out the patient's strength, without offering any prospect of immediate improvement. We meet with cases in which three or four years or more pass away, the parts remaining in an unhealthy state, preventing the patient using the limb or following any profitable employment. On examination, the cancellous texture of the head of the humerus would be found soddened, discoloured, and dead, though not separated from the living bone; or there might be an abscess surrounded by a cyst and hardened bone. In cases like these you may succeed in shortening the period of the patient's sufferings, but pray use caution in your selection, and proceed with care.

Now, when inflammatory disease of the shoulder-joint has run its course, and the patient is pronounced well, it generally happens that the movements between the humerus and scapula are found to be lost. These two bones move as one piece; the deltoid becomes atrophied, and the shoulder presents a flattened appearance. The question will be asked you in practice,—Can you by forcible extension rupture the adhesions, and by subsequent passive movements restore the mobility which has been lost? I have never seen a case in which a favourable result has been thus obtained; and even the late Professor Bonnet who was a warm advocate for this sort of practice, spoke of its success as applied to the shoulder in moderate and guarded terms:—"After the disappearance of the pain which follows these trials (of forcible extension)—an event of speedy occurrence—you must proceed to artificial movements: ameliorations, which *though very limited* have been real, have ensued from this combination of treatment (*des améliorations réelles, quoique très bornées.*)" Nevertheless, Mr. Brodhurst has related a case in which he affirms that much good resulted from these measures. A young woman, aged twenty-four, who had suffered from urethral rheumatism, came under his care, with the shoulder-joint stiff from extra-capsular adhesions. On the 15th of October Mr. Gerrans and he administered chloroform and ruptured the adhesions, which were soft. The motions of the joint were instantly free, and five days later free movement was found.

It fortunately happens that where there is want of success nature compensates the patient for the loss sustained, for the scapula acquires a compensating sphere of action. You will recollect that even in the healthy joint the humerus cannot be raised beyond a right angle to a line drawn through the axis of the glenoid cavity, and the backward movement is very limited. The loss of this amount of movement after

disease is easily borne by the patient. A hairdresser was once under my care whose shoulder-joint became completely and permanently stiff, but after six months he could again raise his hand so as to follow his usual employment.

In conclusion, I would call your attention to a class of cases in which there is complete paralysis and atrophy of the deltoid muscle, commonly a result of disturbance to the nervous centres during dentition. The shoulder loses its roundness; the parts become lax; ultimately the humerus falls from the scapula, and the head of the bone leaves the socket completely. This condition of parts has been observed in cases where the rest of the muscles of the upper extremity have retained their normal power and law of development. Whenever there is loss of nerve power, atrophy or imperfect development ensues; and to this cause must be attributed some of those instances, of which I lately showed you a specimen, in which the whole upper arm is, in an adult, cold, flabby, and four inches shorter than the opposite,—while the forearm, sustained by nerve power, is of its proper size and strength. No treatment is here of avail.—*Lancet, April* 20, 1861, *p.* 381.

41.—ON A PECULIAR AFFECTION OF THE KNEE-JOINT.

By Dr. Francis James Lynch, Physician to the Longhrea Workhouse and Fever Hospital.

[This form of disease of the knee-joint was first noticed by Mr. Mayo, and since by Sir Benjamin Brodie, Mr. Hawkins, Mr. Key, and Mr. Wickham. Mayo in his work on Human Pathology, alludes to it, under the heading of "A class of cases of rare occurrence."]

The disease is mostly observed in persons who have been subject to rheumatism, or to rheumatic gout, and is often brought on by exposure to damp and cold; the patient, after suffering from more or less acute synovial rheumatism of a migratory character, in the wrist, elbows, knees, and other joints, will suddenly complain of intense pain in one knee, where the inflammatory action seems to concentrate itself. The limb is almost always in the extended posture, the heel resting on the bed. The swelling, even at the commencement, is considerable, occupying the knee, the lower-third of the thigh, and the upper-third of the leg; it is not circumscribed above or below, but gradually tapers away, and is lost in the thigh and leg. The skin covering the affected part of the limb has a remarkably glossy, bloodless hue, resembling in colour white marble. It is exquisitely tender to the touch, and the swelling is found to be uniform, firm, and elastic; sometimes a crackling is felt under the examining finger, and at times slight œdema, or pitting on pressure, exists. The joint is the seat of constant pain, sometimes dull and throbbing, at other times lancinating and severe, with frequent and intensely painful exacerbations. The tenderness is very great,

even in the skin, and the least pressure cannot be borne. Moving the toes, or shaking the bed, or walking heavily across the room,—in fact, whatever occasions the slightest disturbance or movement of the limb,—causes intense agony. There does not seem to be much, if any effusion into the joint, and the usual prominences about it cannot be easily detected, owing to the thickened condition of the soft parts, and the extreme sensitiveness of the skin. Frequent spasmodic twitches in the limb increase the patient's sufferings, and interrupt the snatches of sleep, which anodynes, or exhausted nature, at times induce. From the commencement, there is a good deal of febrile excitement; nausea, anorexia, thirst, disturbed rest, quick pulse, heat of skin, and other marks of constitutional derangement, exist; the countenance is expressive of much pain and anxiety, and, as the disease progresses, emaciation, increasing debility, perspirations, and frequently recurring rigors, are observed. In this condition things may remain for weeks or months, when, in the majority of cases, either from the effects of remedies, or, in rarer cases, from apparently the unaided efforts of nature, the urgent local and constitutional symptoms gradually or abruptly abate, and the patient ultimately recovers, with the joint apparently anchylosed; but in process of time a limited degree of motion sometimes returns, only to a slight amount. Ultimately, however, after a year or two, the patient is able to walk without much lameness. The swelling never completely subsides; the patella is more or less fixed; the hollows on either side of the ligamentum patellæ are permanently filled up, not from effusion into the joint, but from thickening of the exterior tissues.

Owing to the limb resting for such a length of time in the extended posture, troublesome sores and excoriations are apt to form on the heel and nates; for months after the subsidence of the acute symptoms the patient is unable to rest on the limb, and its premature use will surely reproduce the original symptoms. Occasionally, during the acute stages of the disease, a degree of tenderness and some fulness is felt in the course of the femoral vessels as high as the groin, where an enlarged gland can be sometimes felt; showing that, in some instances, the venous and absorbent vessels participate in the existing inflammatory action. Such is the progress and termination of the disease, in its milder and most favourable forms; its leading features being well summed up by Mr. Hawkins, in the following extract from one of his clinical lectures on the subject, viz.:—"1st. Very acute tenderness of the skin, resembling, in this respect, hysterical affections of the knee. 2ndly. Great aggravation of pain on moving the limb, or the bed on which they lie. 3rdly. A pale, white, glossy appearance of the skin. 4thly. The swelling is peculiar, differing from that of simple synovial inflammation; for, while the swelling in the latter extends around the joint alone, in this disease it extends around the lower part of the thigh, and the upper part of the leg; it pits on pressure, and you can feel a sensation of crackling beneath the finger."

Mr. Hawkins believes that the disease commences in the periosteum covering the lower end of the femur; and that after involving the areolar membrane external to the capsule, it passes thence to the synovial membrane and cartilages of the joint. At all events, it is probable that the inflammation originates in the areolar and fibrous tissues external to the joint. If, as occasionally happens, the disease is supposed to be rheumatic gout, or common synovial rheumatism, and is treated by colchicum, and afterwards by hydriodate of potash, the patient will suffer for months from the violence of the pain, and the attending irritative fever, escaping ultimately with a joint more or less completely anchylosed, or the cartilages and synovial membrane of the joint become ulcerated and disorganized; and matter forms in and around the joint, running down the patient's strength, and either ending fatally, or rendering amputation of the limb necessary for the preservation of life. In the vast majority of cases, however, there is no tendency to the formation of matter within or around the joint; the cartilages are absorbed; and false anchylosis, without any accompanying suppuration, is established. If the disease be early recognized, rapid mercurialization of the system, and local depletion, will quickly restore the joint to its normal condition; even at a more advanced period, the same line of treatment will, in the vast majority of cases, arrest further mischief, and relieve in a very marked and satisfactory manner the local and constitutional symptoms, which begin abruptly to disappear as soon as the mercurial action is established. The painful condition of the limb pending the mercurialization of the system is often remarkably relieved by local sedative applications, amongst the most potent of which is a lotion of cyanide of potassium dissolved in water (four grains to the ounce), applied tepid. In general, warm applications are preferred to cold; and French wadding, or carded wool, around the joint and limb, with a pad under the heel, will be grateful, and useful. Generally speaking, counter-irritation, by blisters or otherwise, does more harm than good, even on the decline of the disease. When the acute symptoms have wholly subsided, and the patient is able to rest a little on the limb, the Buxton waters, and a judicious course of shampooing, tend powerfully to restore a certain degree of motion to the joint. During convalescence, there are sometimes returns of severe pain, which are best relieved by the cyanide of potassium lotion, or by belladonna—two drachms of the extract in an ounce of water, smeared over the seat of pain.

In the acute stage, the stomach is so much disordered, that opium, in any shape, does more harm than good, when taken internally to relieve pain or produce sleep; but I have found Battley's sedative liquor to be, when a sedative is urgently required, the most preferable form, far more so than any of the preparations of morphia. During convalescence profuse perspirations are common; here the liquor cinchonæ, and eight grains of tannin, every night, will often check this exhausting symptom. A flannel bandage, using very moderate

pressure, from the toes upwards, is useful; but bandaging should not be attempted until *all* pain and tenderness cease. When the disease rapidly disorganizes the cartilages and other textures of the joint, or when a similar result follows a protracted attack, the case assumes the the features of ordinary acute or sub-acute ulceration of the cartilages of the joint, and requires a similar treatment.—*Dublin Quarterly Journal, Nov.* 1860, *p.* 337.

42.—EXCISION OF THE ELBOW-JOINT FOR COMPOUND FRACTURES.

By Dr. GEORGE H. PORTER, Surgeon to the Meath Hospital.

[Although for years, excision of the elbow-joint has been an established procedure in cases of diseased bone, compound fracture of the joint has been quite sufficient until of late to condemn the limb to amputation. The following case will prove interesting to the practical surgeon, as illustrating, in a marked manner, the benefit arising from this operation in a case of compound fracture of the joint.]

George Thompson, aged 35, by occupation a house-painter, and of very intemperate habits, was admitted into the Meath Hospital on the 7th of January, 1860. The history of the accident was briefly as follows:—

He was standing on a ladder about ten feet in height, and engaged in painting a sign-board, a drunken man passing at the time staggered against it, and upset the patient; he came to the ground on his left elbow, which received a compound fracture. When I visited him half an hour after, I found him suffering great pain, and very much collapsed; the joint on examination presented considerable swelling, was unnaturally moveable in all directions, and gave the sensation as if the part were filled with a number of small pebbles. There was a transverse wound one inch in length above the joint, and towards the outside, through this, I distinctly felt the broken humerus, and on introducing a probe, considered that it passed directly into the cavity of the articulation. In consultation with my colleagues, it was decided to enlarge the wound, examine the state of the parts, and if the joint was safe, merely to remove any loose portions of bone, but if otherwise, to excise the articulation, and try to save the limb. Accordingly, having had the patient put thoroughly under the influence of chloroform, I made an incision five inches in length along the centre of the posterior aspect of the elbow, extending an equal distance above and below the articulation; this brought into view the transverse fracture of the humerus, just above the condyles; and on closer investigation I discovered a second fracture passing between the condyles into the joint. I then sawed off with a narrow-bladed saw the broken extremity of the humerus, and having made another incision an inch and a half in length from the middle of the former one towards the external condyle, severing the attachment of the tri-

ceps muscle from the olecranon, I seized the internal condyle in a strong forceps, and carefully dissected it out, all the time keeping the knife close to the bone; in like manner I removed the external condyle. My assistant now forcibly flexed the fore-arm on the arm; I then cleared the soft parts from the olecranon process of the ulna and head of the radius, and rapidly sawed them off. It is curious that throughout the entire operation I did not see the ulnar nerve; scarcely any blood was lost, two small arteries only requiring ligatures. The edges of the flaps were now brought evenly together, and retained by four points of silver-wire suture; the dressing consisted in applying wet lint to the wound, and bandaging the limb on a carefully padded rectangular splint; he was then placed in bed, felt quite cheerful after the operation, and expressed his gratitude for leaving him his arm. Ordered twenty drops of Battley's sedative liquor of opium.

March 12. Wound quite healed; the splint taken off daily for some hours, and the arm merely supported in a strong sling; by giving him a little assistance, he now had the power of flexing the forearm on the arm in a great measure, and when the limb was in the extended position, was able to pull towards him comparatively heavy objects. Nothing worthy of recording occurred from this date until he left the hospital on the 16th of April, at which period his arm was every day becoming stronger. He returned to let me see him at the end of June, when I found him possessing a very useful limb; he had the power of flexion to a great extent, and told me he was able to use the arm on many occasions in his wonted employment.—*Dublin Quarterly Journal, Nov.* 1860, *p.* 314.

43.—ON THE TREATMENT OF CURVATURE OF THE SPINE.

By Holmes Coote, Esq., Assistant-Surgeon to St. Bartholomew's and to the Royal Orthopœdic Hospital.

One of Werner's chief indications as to treatment is so important, and agrees so entirely with the views which I wish to inculcate, that I beg you to commit it to memory. It is that you must bring back the elongated ligaments to their normal length; and he also rightly adds that these ends are not to be obtained by exercises, but by steady mechanical pressure.

I have not time to speak here of arthritic or rheumatic curvatures, but proceed to the important question of treatment.

When a patient whom you imagine to be the subject of lateral curvature of the spine seeks your advice, be careful to examine the spinal column in its entire length. There may be ulceration in the lumbar region, accompanied by consecutive curvative in the dorsal region. This rule even applies to doubtful cases of wry-neck. Last year I

was consulted by the friends of a young lady who was suffering from wry-neck and projection of the right shoulder. There was some talk of the division of the tendon of the sterno-mastoid muscle, a proceeding by no means free from danger. On examination, however, I found a considerable amount of ulcerative disease of the bodies of the lumbar vertebræ, and angular curvature of the spine in that region. The projection of the shoulder was consequent on this alteration of form; the wry-neck was due to irritation of the spinal cord. I corrected the deformity of the neck, which was a subject of serious annoyance, as well as I could by apparatus, but declined to interfere by surgical operation. The patient went to the sea-side with proper instructions; but the accuracy of the diagnosis was shown by the fact that since the period when I last saw her she has suffered from lumbar abscess.

Having ascertained that the patient is free from any such complication, proceed in the next place to ascertain that there is no physical defect, such as inequality in the length of the lower extremities—that there is no rhachitic deviations from the proper form; inquire minutely into the patient's habits; note whether there be short sight or any disparity between the two eyes. Having arrived at the conclusion that the deformity is due to general debility, you proceed to give directions by which the elongated ligaments may be allowed to contract, the shortened ones become elongated, and the bones be pushed back to their proper relations. In very many instances this may be accomplished if the treatment be commenced early; but what hope of cure can be reasonably entertained when the vertebræ have become altered in form?

The mistake commonly made is that in incipient cases the muscles may by proper exercises rectify the impending deformity. Will muscular action cure a knock-knee or flat-foot? Why should the effect be different in one articulation as contrasted with another? What benefit can accrue from the wearing of a weight on the top of the head? The spine is already too weak to sustain the natural burden imposed on it.

You may meet with many cases in which it is said that localized movements, muscular exercises, gymnastics, and calisthenics have effected a cure; but upon minute inquiry you will generally find that, combined with these measures, which as accessories are both useful and healthful, there is usually abstinence from some habit detrimental to upright growth. Girls are kept often many hours a day cultivating an accomplishment which they give up when married, such as drawing, or harp-playing. The shoulder grows out slightly. Abstinence from the injurious occupation, healthful exercises, a few months' growth, and the slight increase in *embonpoint* may render the spinal deviation almost imperceptible. Such measures cannot remove it, but may cause it to remain stationary. What that person's future may be will depend on circumstances. Suppose that she becomes a mother, and has to nurse a family of heavy children, the evil returns, and

that at a period in life when instrumental support is impossible. I have a lady under my care, aged 42, in whom this deformity has rapidly increased during the last ten years of her life. Health and spirits are quite gone. She has lost all moral courage, and while suffering severely from the sinking of the side, malformation of the chest, and visceral displacement, has not the fortitude to bear the least inconvenience which every instrument must in some measure cause. Those changes in the heart and lungs which I have already described are now taking place—namely, dilatation of the right cavities and pulmonary emphysema, and if she were called upon to exert herself for a living, she would be in danger of falling down dead.

There is but one way of correcting spinal curvature,—namely, by mechanical support and direct pressure. That pressure must be unremitting, commenced early, and maintained at the proper standard for many months, perhaps for years. The inconveniences attendant on such treatment are doubtless considerable; but what have we to put in its place? Will the general health bear confinement in the prone position on a couch? And as regards localized movements, they may pull upon and even stretch contracted ligaments, but they cannot guard against their recontraction. But in nearly every case there is a stage in which a moderate amount of support will effect all that is necessary. When the spine first begins to yield, the curve being periodical as it were, and coming on only in certain postures of the body or after fatigue, the greatest possible amount of benefit may be obtained by the wearing of stays provided with lateral supports. These side-crutches, introduced so as to present no unsightliness nor demand alteration in the dress, serve to maintain the spine in the upright position. The stays, called "French stays," fix in front, but can be tightened to any degree by strings which pass from behind forwards and then round the waist. Instructions must be given that the patient should rest upon the sofa whenever the back begins to ache; and with this mechanical treatment you may combine the administration of tonics, proper out-door exercise, and any well-regulated amount of calisthenics.

The more decided curvatures, however, are to be treated only by powerful apparatus, weighty and strong; if it were not so, it could exert no active influence on the spine, which would press the instrument itself back, instead of yielding before it. It is constructed on the same principle as Tavernier's belt, but is incomparably superior, the pressure being exerted by pads attached to steel rods moved with cog-wheels. The object which you have in view is to raise the shoulder which is depressed; to push as far as possible into place, by gentle yet unremitting force, the displaced bones by acting on them through the ribs, and to maintain them in their right position until the tendency to curvature has passed away. During all this time the patient may take the usual exercise and follow her accustomed avocations. But let me give this advice: Do not undertake the manage-

ment of a confirmed case unless it is agreed that your attendance is to be very frequent, and extended over twelve months at least; and, moreover, do not allow the patient or the friends to take charge of the instrument. If these rules be neglected, disappointment will follow, and the blame attached to want of success will rest upon you.

The apparatus, now well known, and called by the name of the gentleman who designed it—Mr. Tamplin,—may be worn by infants as well as adults. Apparatus somewhat similar have been designed by Messrs. Adams and Brodhurst.—*Lancet, March* 2, 1861, *p.* 209.

44.—INSTRUMENT FOR DISLOCATIONS.

Dr. WATSON exhibited an instrument, which he owed to the kindness of Dr. Keith, of Aberdeen. The instrument was invented some years ago by a student of the Aberdeen Infirmary, who, following up a suggestion of Dr. Keith's, that a great desideratum in reducing dislocations was some means by which the extension force could be suddenly slacked off so as to admit of the muscles asserting their influence to rectify the displacement. This instrument most completely fulfilled the indication of "holding fast as long as was desired, and of letting go at once when required." It consisted of an ellipse of steel, which could be attached to the hook of one of the pullies of the extending tackle, by a hole in an arm projecting from and continuous with one side of the ellipse. The ellipse itself was thus composed of two pieces hinged together at one end, so that the other extremity could open or close like a pair of "*shears*" or compasses. Near the open end of the movable arm of the ellipse was a hook, which, when the two arms were in contact, caught upon a pin projecting from the side of the other arm, thus binding them firmly together; so that, unless the hook was displaced from its hold upon the pin, the ellipse of steel remained complete, and the ring, which might be placed within its curve and hooked on to the "laque," or extending straps, could not escape from its embrace. In order still further to keep all firm, and prevent any lateral displacement of the hook, a side spring pressed it flat against the arm upon which the pin was fixed. So arranged, the instrument could hold fast against any force which was not sufficient to bend the hook or break the pin. When, however, it was desired to slip the extension force, this could be instantly effected by a small lever hinged close by the pin. Its long arm stood at right angles to the rest of the instrument so long as the extension force was applied, and its short arm, of a wedge form, worked beneath the spring and elevated it, while it pushed against the hook and disengaged it when the long arm was depressed. Thus the extending force could at once be let slip from the limb without the risk of entanglement or of a moment's delay. Dr. Watson had been fortunate enough to see Dr. Keith employ the instrument in a case of dislocation of the hip-joint. From observation, he could testify that, whether actually

useful in effecting the reduction of a dislocation, by enabling the surgeon to let go the extension force suddenly, it certainly appeared to be extremely convenient as a means for *instantly* slacking away the tackle when the dislocation had been reduced.—*Edinb. Med. Journal, March* 1861, *p.* 839.

45.—*Osseous Anchylosis of the Elbow in a Faulty Position.* (Under the care of Mr. Henry Lee.)—We have several times drawn attention to the good results which ensue in the treatment of anchylosis of the elbow and other joints by the forcible rupture of the uniting medium, whatever that may be, provided it is not bony. Flexion of the joint is not to be thought of when osseous union has taken place. The question then arises, what is to be done in a case of anchylosed elbow in a faulty position? An example in point was recently admitted into King's College Hospital, under the care of Mr. Henry Lee, in the person of a delicate-looking girl, about seventeen years of age, who had long been a sufferer from possibly strumous disease of her right elbow, which in the course of time had become anchylosed in a straight position. There was no motion in the joint, the various bones seeming to be fused into a solid mass. Mr. Lee determined to put into practice a plan, the merits or demerits of which experience has yet to decide upon—namely, to saw through the lower end of the humerus above the joint, bend the arm, and obtain anchylosis in an improved position. Accordingly, on the 6th of October, chloroform was administered, an incision made at the back of the joint, and the lower end of the humerus exposed and sawn across by means of a circular saw and concave blade, the latter being placed under the bone. By rotating the saw with the handle, the bone was quickly cut through. The arm was now flexed after closure of the wound, and the patient removed to the ward.

In the event of anchylosis again occurring by the aid of callus, it seemed extremely questionable whether the girl would be in a better condition than she was previously. A false joint, it appears to us, should have been aimed at, and this result has actually happened, a useful arm being gained by the very measure which was intended to give her a stiff arm, only in a better position than it was before.—*Lancet, March* 30, 1861, *p.* 314.

ORGANS OF CIRCULATION.

46.—ON A NEW MODE OF DELIGATING THE FEMORAL ARTERY.

By George H. Porter, Esq., Surgeon to the Meath Hospital.

[The father of the author many years ago showed, that when, after ligature of the femoral artery for aneurism, gangrene occurs, it is

usually occasioned by the vein also becoming obstructed by the size of the tumour.]

But he regarded a wound or injury of the vein a source of great and often unavoidable danger in the operation as usually performed, and held the principle that no proceeding could be considered as safe and eligible, when such risk was incurred. In the second number of this journal, for May, 1846, this part of the subject has been extensively discussed, and, therefore, it is needless to dwell farther on it here; but it may be observed that, if it be true that in the best hands, and with the greatest caution, not only may the vein be wounded, but that sometimes it is impossible to avoid it, and if the enclosure of a portion of that vessel within the noose of the ligature be inevitably fatal, surely the operation exposed to such contingencies cannot take a very high place amongst the resources of surgery. Actuated by these considerations, he proposed that the usual place selected for securing the femoral artery should be abandoned, and another chosen, which should at least be free from the last-named formidable objection.

After the artery has passed under Poupart's ligament, it lies quite superficial for nearly an inch of its course, accompanied by its vein, which is to its internal side, and just as superficial, and on a level with it. Any incision, therefore, by which this latter vessel could be endangered, must so far expose it to view as to render its avoidance easy, and any operation performed on this portion of the artery must be free from the objection above stated. Indeed, it is difficult to conceive how the vein could be injured, unless by the most absurd and culpable recklessness. But in this part of its course it gives off numerous branches, and the vicinity of any one, even of the smallest of these, would interfere with the formation of an internal coagulum, and with the subsequent process that ought to lead to the obliteration of the vessel. True, it does give off these branches immediately while passing under Poupart's ligament, or shortly afterwards; it gives off the circumflex ilii and epigastric, two large and important branches, quite sufficient to mar any operation, if this objection were really effective; but these branches would be above, or at the cardiac side of the ligature, and experience proves that secondary hemorrhage does not take place in such situation. Moreover, there seems no reason for supposing that these branches would be more injurious in the case of a ligature placed below them, than would be the profunda in the old operation, which was always left above the cord in order to preserve the circulation of the limb. Lastly, the common carotid artery had been tied within a quarter of an inch of the bifurcation at the arteria innominata and the subclavian at the edge of the scalenus muscle; and, assuredly, if the presence of a large branch at or near the cardiac side of a ligature would prevent the obliteration of the vessel, such operations as these could scarcely have proved successful.

But the femoral artery gives off, lower down, certain small and irregular branches—the external or superficial pudic, for instance—some of which must be below, or at the distal side of the ligature, the very situation where hemorrhage occurs, and where the presence of one ever so small may thus prove destructive. It may be so, but it has never been proved. On the contrary, where secondary bleeding has happened, the cause has always been in the ulceration of the artery at the ligature, and this, whether there was a collateral branch in the vicinity or not. There has been no lymph, no adhesive inflammation, no attempt at occlusion, although the space of an inch might intervene between the cord and the nearest branch, whereas in the many operations that have been successfully performed it seems impossible that all must have been so fortunately circumstanced as that not one should have had even a small or trivial collateral twig in the vicinity of the ligature at its distal side.

Supported by these considerations, it was suggested that the artery might be deligated safely and successfully at a point about half an inch below Poupart's ligament, where it lies superficial, where neither vein nor nerve could be exposed to danger, and where the collateral branches would be at a sufficient distance not to interfere with the subsequent process of obliteration, if such was really effective; and it was also proposed that the operation should be performed by a single incision parallel to Poupart's ligament, and of course transverse to the vessel, such incision giving the least disturbance to the artery in separating it from its adjacent connexions, and affording equal, if not greater facilities for passing the cord around it, and placing the wound in the most favourable position for healing subsequently. All that was required to insure success was, that the patient should be prepared for the operation for a few days by confinement to bed, a moderate diet, and gentle aperient medicine; that he should be kept during the whole progress of the cure in a pure and wholesome atmosphere; and that, until the perfect cicatrization of the wound, he should observe the most absolute and perfect quietude. The importance of this latter condition will be made very apparent hereafter. At the time, however, these suggestions met with small encouragement. Few surgeons agree in the pathology of the arterial system, and fewer still could be found willing to leave the beaten track, and adventure their own reputation and their patient's life on an operation wanting the sanction of experience. In the meantime, a case of popliteal aneurism was cured in the Richmond Hospital by mediate compression; another occurred in Jervis-street Hospital, and soon this method of treatment came to be ratified and confirmed by a course of success so unvarying and continued, that most surgeons began to regard the operation of securing the femoral artery as little more than a matter of bygone history, never more to be resorted to in the treatment of popliteal aneurism.

But in medicine there is no proposition universally true,—no mode

of treatment applicable to every possible case. Doubtless, the operation of deligating the femoral artery at the usual place has totally fallen into abeyance, and probably has not been twice performed in Dublin during the last fifteen years, whilst the treatment by compression has been generally so successful, and has so entirely won the confidence of the profession, that very good and sufficient reasons would be required of any practitioner proposing to deviate from it. Such cases, nevertheless, must occasionally occur; sometimes the vessel may be so circumstanced that compression cannot be satisfactorily applied or maintained; sometimes the pain is so great (for it cannot be denied that the process is occasionally very painful) that the patient will choose any risk of operation rather than endure it; and sometimes we meet with persons so wayward and ungovernable, that no temporizing method can possibly be carried out with them. I have seen two cases in which, after compression had been tried, and failed, amputation was necessarily had recourse to, and in one of which the operation proved fatal. To meet such exceptional emergencies, the operation of taking up the femoral artery must be resorted to, and the sole question for discussion is, whether, in such eventuality, the operation I am about to describe is likely to be attended with less hazard than the old one, as performed in Scarpa's space. Perhaps the best description I can give will be a detail of the first case in which it was performed: I extract it from my father's case-book, and give it in his own words:—

John Neile, aged 40, by occupation a farmer, and of very healthy appearance, was admitted into the Meath Hospital, October 18, 1849, with a large pulsating tumour, occupying the middle and internal part of the right thigh, extending upwards to within a hand's-breath of the groin, and downwards to within the same distance of the knee. Its measurement, both in the longitudinal and transverse directions, as far as could be ascertained, appeared to be about six inches; and the circumference of the affected limb, where the tumour was most prominent, three inches more than that of the other. Pressure on the artery, above, arrested the pulsation, and caused the tumour slightly to collapse, but it still remained very soft, and its contents were evidently quite fluid. There could be no doubt of its being an aneurism, and its history showed it to be one of no trifling importance.

On the morning of the 20th the femoral artery was secured in the following manner:—

An incision, about an inch and three-quarters in length, was made *across* the direction of the artery, at the distance of half an inch below Poupart's ligament, and exactly parallel to it; this first incision very nearly exposed the vessel sufficiently, only a few touches of the knife being required to free it from its connexions, and allow the needle to be passed easily around it. Scarcely a teaspoonful of blood was lost, and he seemed to suffer very little until the moment the ligature was

tied, when he complained of a pain shooting down the entire limb to the foot; it was, however, only momentary. On the artery being secured, the wound was closed with a strap or two of adhesive plaster, the limb bandaged with a flannel roller, and the patient replaced in bed; the whole operation having occupied an incredibly short space of time.

[The ligature came away on the nineteenth day, and the patient left the hospital cured about eight weeks after admission.]

After relating another case the author observes:—

I confess that the point of interest which has induced me to publish these cases, and to which I wish to direct the attention of the profession, has reference to the question of secondary hemorrhage, that object of terror to all operators, and which has really proved such an obstacle to success. All surgeons of the present day believe that the vicinity of a collateral branch (however small) to the ligature, vitiates the process of union by preventing the effusion of agglutinative lymph; and, as far as I know, this is the only objection that can be raised to principle of those operations I have just detailed. Mr. Erichsen, who may well be accepted as the exponent of British Surgery, in his splendid work, "The Science and Art of Surgery" (London, 1853), when treating of tying the femoral artery between the inferior edge of Poupart's ligament and the origin of the profunda, says:—"Of twelve recorded cases, in which this artery has been tied, it would appear that three only succeeded, whilst in the remaining nine instances, secondary hemorrhage occurred, which proved fatal in three, and in six was arrested by the ligature of the external iliac. This operation, I think therefore, ought to be banished from surgery; and in all those cases of aneurism that are situated above the middle of the thigh, and in which sufficient space does not intervene between the giving off of the profunda and the upper part of the sac, for the application of a ligature to the superficial femoral, the external iliac should be tied, unless compression can be employed."

Certainly it requires no little confidence to question a doctrine thus authoritatively advanced, and universally received. But I must remind my readers, that up to this moment the operation on the femoral artery in this locality has always been exceptional, never adopted through choice—never resorted to unless when some strong objection existed to the selection of Scarpa's space, which objection may be admitted as adverse, more or less, to the success of any operative proceeding. And in the cases alluded to by Mr. Erichsen, as far as I have been able to analyze them, this remark is fully borne out. Again, in the recorded cases of hemorrhage, I do not find any one instance proved to have been occasioned by the vicinity of a collateral branch; such vicinity may have existed, and probably did, but its injurious influence has in no case been demonstrated. As far as I know, destructive consecutive hemorrhage is always accompanied by the presence of unhealthy ulcerative inflammation and the absence of lymph; but I am not aware that these results have been traced to

the presence of a collateral branch. It is very true, that when such branch exists, there will be no coagulum of blood within the vessel. Its absence may have originated the idea that the cause which prevented its formation, also prevented the deposition of coagulating lymph, but certainly the two results can have no connexion; and that they have not, is, I think, fully proved by the cases just related. There can be no question that two large collateral branches lay closely above the ligature, and that a layer of lymph was formed nevertheless. This lymph was so thin that the pulsation of the trunk of the vessel could be felt into the very wound, and so weak that it yielded and broke under the excitement of a violent cough, and permitted the escape of a considerable quantity of blood. This took place in both instances, but the lymph was adhesive and healthy, the bleeding easily controlled by slight pressure, and perfect recoveries the result. So long back as the year 1813, Mr. Travers noticed that "secondary hemorrhage sometimes results from the laceration of the young and tender cicatrix;" but, says he, "this is not to be classed amongst its natural causes." Now, if this can happen at the cardiac side of the ligature, when the young and tender cicatrix has to meet and stem the full stream of blood under the strong power of the heart's action, why may not the same happen at the distal side, or why should a collateral branch have an influence in one situation which it clearly does not possess in the other. The answer probably is, that the cause of the hemorrhage is not in the presence of the collateral branch, but in the kind of inflammation set up in the vessel; it is plastic, where it is to be obliterated; it is erysipelatous and ulcerative, where it bleeds. I am quite aware how little reliance can be placed on experiments on the lower animals for the elucidation of human pathology, and especially with regard to the arterial system; nevertheless, there is one circumstance that seems to bear strongly on this point under consideration. I do not believe it possible to produce secondary hemorrhage in the inferior animal. We may ligature what vessel we please, and where we please, near to or at a distance from a collateral branch, and either above or below it, and the result is always the same, the the perfect closure and obliteration of the vessel. But the inflammation in the lower animal is always healthy, always plastic; and whenever in the human subject it bears the same character, I believe it carries the same result. If this be the case, then, and it is a question of pathology which any inquirer may determine for himself, it will remove the chief, if not the only objection to an operation which is in other respects simple, comparatively safe, and easy of performance, free from pain and bleeding, and in which a wound of the vein is well nigh impossible. It is not pretended that secondary hemorrhage will not occur; to be sure it will, but not in this case more frequently than in any other, not from the cause to which it has been most frequently attributed, and certainly not in a more hopeless and irremediable form than where the artery is more deeply placed, as it is in the old operation.—*Dublin Quarterly Journal, Nov.* 1860, *p.* 302.

ALIMENTARY CANAL.

47.—ON THE RADICAL CURE OF REDUCIBLE HERNIA.

By James Syme, Esq., Professor of Clinical Surgery in the University of Edinburgh.

[The subject was introduced at a meeting of the Medico-Chirurgical Society of Edinburgh, Professor Syme at the same time showing a patient on whom he had operated with success. Mr. Syme considers the only objection to Wützer's operation to be, the complexity of the apparatus required. The plan now brought forward he considers more simple, and perhaps more effectual than Wützer's operation.]

Mr. Syme would first remind the Society that M. Wützer's operation consisted in invaginating or pressing up a piece of integument so as to cause it to occupy the inguinal canal, and in retaining it there by means of a needle until adhesions had formed, when the invaginated part acted like the cushion of a truss, and confined the bowel within the abdomen. The apparatus by which this was accomplished was very complicated. When its different parts had been put together, the instrument (smeared with an irritating ointment) was introduced up the external ring; a long needle was passed through it, so as to penetrate the invaginated integument and the parietes of the abdomen, where it was retained by means of a guard; after which, another part of the instrument was employed to compress the parts concerned. Not only was the apparatus very complicated, but it was by no means easy or certain to introduce the instrument fairly within the external ring.

Since this method had been proposed, various modifications of the apparatus had been suggested, but certainly not with the effect of simplifying it.

The plan which Mr. Syme had now to recommend was of the simplest character. Instead of a complicated apparatus for filling up the inguinal canal, the surgeon only required an elongated body of some kind, such as a piece of bougie, a piece of an œsophagus tube, a bit of wood, or even part of a wax or tallow candle; all the preparation required was, that a hole should be drilled through one end of the body chosen. The remaining apparatus consisted of a piece of strong thread and a needle, such as is used in sewing up dead bodies. The mode of application was as follows:—The string was passed through the hole in the end of the body; the needle was threaded with one end of the string, and laid with its concavity resting on the forefinger of the left hand, which was then passed up along the cord within the external ring; the needle was then turned round, so as to bring its point upwards, and passed, with an inclination to the left, through the textures, and brought out on the surface of the abdomen; the other end of the thread was then passed through the parietes in the same way;

only that this time the inclination of the needle was to the right; the two threads were then pulled tight enough to draw the body up the canal, and up it must go. The piece of tube, as in M. Wützer's operation, was smeared with cantharides ointment, to irritate the skin and favour the formation of adhesions. The two ends of the thread were then tied together; and, to prevent any chance of cutting through the skin too fast, a bit of elastic bougie was placed below them. For three or four days, or a week, a compress was placed over the groin, and retained in position by a bandage; and this was the whole process.

Mr. Syme presented to the Society a patient on whom the above operation had recently been performed. He was a seaman, 21 years of age, and had suffered from inguinal hernia for two years. The aperture was wide, and the tendency to protrusion very strong; it was, therefore, in every way very desirable that a radical cure should be effected. The patient was admitted on the 30th of last January; the operation was performed on the 1st of February; the tube was removed on the 11th; on the 25th the patient was up and walking about the ward and had done so ever since. No truss had been worn till this evening; but, as the patient had some distance to come, Mr. Syme had thought it advisable to use the precaution of having one applied. The members of the society might satisfy themselves that there was now no tendency to protrusion, no impulse on coughing, and that the parts concerned felt firm and indurated. The object of the operation had, in short, been perfectly attained, and the finger could be introduced nearly to its whole length up the invaginated portion which still retained its place. How far the cure in such cases would be permanent, Mr. Syme was not prepared to say; M. Wützer must be responsible for the permanency of the result. What Mr. Syme had done was to facilitate the operative procedure, and to attain M. Wützer's object in the simplest way, and with means which every surgeon had in his possession. An objection had been made to the operation on the ground that it excited too much irritation; but this very irritation was useful in exciting such an amount of adhesive action as was required.

Mr. Syme added, that the above was not the first case in which he had operated; but that he had done so repeatedly, and in no case with any bad effect.—*Edinb. Med. Journal, April* 1861, *p.* 865.

48.—ON WUTZER'S OPERATION FOR THE RADICAL CURE OF REDUCIBLE INGUINAL HERNIA.

By T. Spencer Wells, Esq., Surgeon to the Samaritan Hospital.

[Several surgeons who have tried Wutzer's operation lately, complain that though for a time the cure seems complete, yet that after a short period the hernia again descends; in fact that the operation is quite

a failure. Mr. Spencer Wells first introduced Wutzer's operation to the notice of the profession in England, and he again desires to reiterate his opinion that in suitable cases it is simple, safe, and perfectly successful. Want of success is due to certain well ascertained causes which may be classed under the following heads.]

I. The performance of the operation in cases for which it is not suitable.

II. Imperfect performance.

III. Want of precaution in after-treatment.

I. In my first paper on this operation, published in the Medico-Chirurgical Transactions of 1854, I expressly limited the class of cases in which it is indicated ot "strong, otherwise healthy persons, up to 40 or 45 years of age, who lead a life of active bodily exercise." I said we might depend upon a radical cure in such patients "when the hernia has only acquired a moderate size, has not become adherent, and when the long diameter of the inguinal canal has not been much shortened by the continued pressure of the intestine." I showed that in other cases, where a radical cure could not be depended on, patients whose hernial tumours could not be kept up by any mechanical assistance were so much benefited that the hernia might be afterwards properly kept up by a truss. In a lecture published in the Medical Times and Gazette in January, 1858, and in a paper published in the Dublin Quarterly Journal of Medical Science in May, 1858, I offered some reason for hoping that this limitation of the operation had been too strict. Mr. Jones, of Sussex, had a successful case in a man 63 years of age. I had one in a patient, aged 59; and on the other hand, Mr. Tamplin operated successfully on a child only 2 years old, and Mr. Bickersteth, of Liverpool, on a boy of 9. These cases, however, did not induce me to perform or recommend the operation on children,—for in these a well-fitted truss may be hoped to lead to a radical cure —nor as a rule to recommend it in very old people, other than as an auxiliary to the truss. In the paper in the Dublin Journal, after giving an account of Rothmund's experience at Munich as to relapse, I state that it "confirmed my previous impression, namely.—that in cases where the canal is not larger than to admit the finger easily, the radical cure may be relied on with almost absolute certainty, and that the probability of relapse increases with the size of the canal and rings. When the rings are very large and the canal very short, the chief use of the operation is to make a truss effective." My subsequent experience has completely confirmed the opinion that if these rules be adhered to, and the operation be performed only in cases of moderately large rings, and moderately long canal—(a ring, for instance, which does not admit more than two fingers, and a canal at least a clear inch in length)—success may be attained in a very large proportion of cases. If the ring only admit one finger and the canal be at least an inch and a-half long I think success is almost certain. I am quite aware

that not one in ten cases of hernia that come before the surgeon fulfils these conditions, and that its restricted application detracts very greatly from the value of Wutzer's operation. But it does not in the least detract from its value in suitable cases. The value is restricted, but not more so than its author represented, and the error is not his, but the error of those who have expected too much from it and have performed it in cases where a radical cure could not reasonably be expected from it.

II. Published reports of cases, and my own observation, convince me that the operation has often been performed imperfectly. In some cases that I have seen, the cylinder had never been within the ring at all. In others, there had not been the necessary adaptation of the size of the cylinder to that of the canal. In some, no attempt had been made to produce an equable compression by the cover, which only pressed near the needle puncture, or else along one edge, making a deep indentation, or sometimes causing a slough. More than once I have seen the instrument bound tightly down by a bandage, until so much swelling of the chord and pain were produced that the cylinder had to be withdrawn. In some promising cases, harm has been done by removing the cylinder too early; and in others by leaving it too long. All these are points of practice which have an important influence on the result, and which my papers, before alluded to, explain fully.

III. In the after treatment, the general error appears to meto have been with regard to the truss. Some surgeons look upon the use of the truss for three or four months simply as a mask of failure; others say it is injurious, as it leads to absorption of recent exudation; both, accordingly, neglect it. I believe, on the contrary, with Wutzer and Rothmund, that it is essential to permanent success. I explained in the Dublin Journal that the truss must be very elastic, "with a weak spring, and large, well-stuffed pad. If the pressure be too great, or the pad too small, absorption of the plug may take place, and relapse of the hernia follow. If no truss be worn, the adhesions which are still soft and yielding might give way." I have operated with complete success on three patients who had previously been operated on by other surgeons; one of them had never worn a truss, nor had the least precaution been taken after removal of the instrument. In another case a very strong truss, with a conical pad, which the patient had worn before the operation, was used after it. In the third case it was quite evident that the cylinder never could have been within the canal. But I must refer those interested in the subject to my lecture and former papers for these details.

The attempts to effect a radical cure of hernia in England have recently undergone two of the usual changes in the history of all improvements. When first made known here, Wutzer's operation was received with indifference or distrust. Then, as these feelings

were overcome, a period followed of exaggerated expectation or confidence. Disappointment was the natural consequence; and a variety of new methods have since been tried, all more or less dangerous, some of them having actually led to death, and others to very serious illness. Five years ago it was difficult to convince an English surgeon that any operation for the radical cure of hernia was justifiable in any case. Now it seems to be the fashion to experiment on every case of hernia. Either extreme is equally irrational; but the latter is by far the more dangerous. I have seen cases operated on where I am quite sure that had the surgeon himself been the patient, he would have infinitely preferred to trust to a truss; and when there was nothing in the occupation of the patient which would lead to the conclusion that he ought to be treated upon different principles to his surgeon. It is a sound principle of surgery that it is wrong to perform any operation which is at all likely to endanger life merely to save the patient from inconvenience or remote danger. Now, some of the operations recently performed are really dangerous operations; they have actually been followed by death; although the number of cases is not yet very large. But not one of the surgeons who have performed Wutzer's operation has seen any dangerous symptom, still less death, produced by it. It has been perfectly and permanently successful in the hands of many surgeons, and must continue to sustain this character if carefully performed in suitable cases. What now remains to be done is to find some equally safe operation for those cases of wide rings and short canal for which Wutzer's operation is not suitable. This will probably be done by some simplification of Mr. Wood's operation, substituting, as he has recently done, wire for the twine he used in his earlier cases. I have made trial, 1, of Schuh's method; 2, of simply passing a wire seton through the canal; and, 3, by fixing the invaginated scrotum within the external ring by one of the large pins sold in the shops as the "safety shawl pin," passed through the plug and both pillars of the ring, and fastened over a compress of linen held in its place by the fastening of the pin. When sufficient time has elapsed to enable me to speak of the results with confidence, I hope to be able to bring them before the profession. But I do not feel disposed to imitate those gentlemen who object to *Wutzer's* operation because relapse has occurred several months after its performance; and then bring forward cases as *successful* a few weeks only after *other* operations have been performed.

In conclusion I may allude to the curious fact that I have been consulted in cases where the operator believed that his operation had failed, because the cutaneous portion of the scrotal plug had descended to its former position, whereas I have been enabled to gratify both surgeon and patient by the assurance that the cases were completely successful, the fascial portion of the plug completely occluding the canal and effectually preventing any protrusion.—*Med. Times and Gazette, Jan.* 19, 1861, *p.* 58.

49.—ON THE RADICAL CURE OF HERNIA.

By Dr. JAMES MORTON, Surgeon to the Glasgow Royal Infirmary.

[The patient was a labourer, admitted for another complaint. He was found to have a very large scrotal hernia on the right side. It was oblique and reducible. The inguinal canal very much distended, and straighter than natural. He complained of great inconvenience from the hernia, and operation was determined upon. On August 4, a metallic ligature was passed subcutaneously around the sac, at the lower part of the inguinal canal, excluding that part only which lay behind the cord, and fixed to a leaden plate by a nipple. A compress and bandage were then applied, and the patient was directed to keep the recumbent position. A grain of opium was given. The patient was discharged on the 20th August, the intestine still filling the inguinal canal when he coughs, but not passing any further, and only coming so far when he coughs much. Next month he reported that he had not enjoyed so much comfort for years.]

The operation performed in this case was as follows:—The contents of the hernial sac being returned, the patient, in the recumbent position, was put under chloroform; the sac was then grasped by the finger and thumb, and slightly raised; a handled needle, with the eye near its point, and furnished with a wire ligature, was introduced under the skin, and guided round the sac so as to include the greater part of the sac, if not the whole, care being taken not to include the cord; and the end of the needle being brought out as near to the point of its entrance as possible, the ligature being caught and pulled out, the needle is withdrawn.

It is plain that the needle cannot be brought out precisely where it was first inserted; and, in consequence, it is requisite to pass the end or ends of the ligature (for in this case I used a double one) again into the eye of the needle, and pass it under the skin so as to get the ends of the ligatures brought out at the same point, and there they may be tied. To give a fixed point, as well as to prevent the drawing inwards of the ligatures, I used in the above case a very small metal plate with a single nipple, over which the ligature used was fastened. I have said that I used a double ligature; and for this reason chiefly, that, in event of one being rendered unserviceable by the formation of a *kink*, I might use the other without being compelled again to use the needle. Besides, it occurred to me that the additional irritation produced by the second wire would not be injurious, but the reverse, and possibly not more than would be required for agglutination of the distended parts. Care must be taken that the ligature does not include the cord or vessels; and during the operation this can be be ascertained by gently pulling the wire when the included parts are raised, and the cord can be felt below. A few days after its application, the ligature is removed. A little water dressing, or one equally simple, may be employed.

The advantages of this mode of operating are, chiefly, its simplicity, it causing little or no suffering, its effectually closing the hernial sac and causing its opposite sides to adhere, and that comparative freedom from dangerous or disagreeable consequences which is well known to accompany subcutaneous operations, and even other lesions of various parts, when the skin remains sound, or nearly so. It commended itself to me by its apparent safety, and this has been verified by at least one case. In the aged it must be difficult to procure adhesion by pressure; whereas ligature would be certain to do so, and the risk of peritonitis almost infinitesimal.

The objections which can be urged against the more common modes of operating induced me to adopt the above procedure. These objections we may shortly state. There is, first, the employment of a complicated instrument, which is always an objection so far as the operator is concerned; secondly, this instrument causes a considerable amount of very severe suffering, and in some cases gangrene of the parts has resulted; and, thirdly, when using this instrument there is always an invagination of skin and sac, and this is said to be liable to be pulled away from the inguinal ring by its own weight or other connections when the instrument sustaining it is withdrawn. It is right to add, however, that operations with such instruments have been frequently successful; and that, so far as known, scarcely a fatal case has occurred. At least one case is known to me in which the patient was considerably worse after than before the operation; and some statements regarding their success on the continent have been questioned by British authors. The operation which I performed in the case now detailed may not be quite so safe in cases of femoral hernia as I conceive it to be in inguinal cases. In the former, the femoral vein is so closely applied to the hernial sac at its point of exit from the abdomen, that there may be some risk of wounding the vein in attempting to pass a sharp-pointed needle around the hernial sac.

Since the time when I operated on the above case, it has occurred to me that in some cases, especially in such as presented a large inguinal opening, the following plan might be attended with some advantages, viz., to make a valvular opening in the skin, and use one or more staple sutures. To make this more plain, let me state more fully what is meant by it:—An incision may be made through the skin a little under the abdominal ring, or point of exit of the bowel; the skin could then be easily drawn up so far as to enable the surgeon to reach the sac at the ring, when he might pass one or more staple sutures, according to the size of the opening; the free ends of such sutures lying downwards, and coming out by the original incision in the integument; these ends could either be tied close to the sac or fastened externally to a plate of lead, and removed when they had served the purpose of the operator. Should an opportunity offer, I mean to put this proposal into practice. It does not seem to me much more hazardous than the one which I have already tried; and it is

worthy of remark, that if the attempts which have been made towards the radical cure of hernia have established anything it is this, that the peritoneum, one of the most important of the serous membranes, can be perforated, at least in its diverticula or continuations, with almost unvarying impunity.

It will be perceived that the preceding case is not brought forward as a perfect cure, though such it may yet prove to be. The ligature, as thus applied, does not close the ring, but the sac; and the patient will require a truss for a time. The operation, however, seems to be as safe as it is simple (two most important recommendations) and it very materially improves the condition of the patient.—*Edinb. Med. Journal, Dec.* 1860, *p.* 515.

50.—SOME SUGGESTIONS FOR AN IMPROVED PRACTICE IN STRANGULATED HERNIA.

By T. Bryant, Esq., Assistant-Surgeon to Guy's Hospital.

[Mr. Bryant shows how few cases of femoral hernia are reduced by the taxis as compared with inguinal, and how frequently, a recently strangulated inguinal hernia may be reduced when the patient has been been brought completely under the influence of chloroform.]

Having shown the benefit which the use of chloroform has already conferred on patients suffering from strangulated hernia, the author proceeded to consider how far the recognition of the truth should influence our practice, and in what way we can turn the information to account. The author stated, that to his mind, the lesson to be learnt was not a difficult one; for if it be true that we possess in chloroform the most efficient means for producing complete relaxation of the muscular system, and with it, of the parts involved in hernia, and if it be acknowledged that a patient can be more speedily and effectually brought into that relaxed and desirable condition by such a method than by any other which we have been in the habit of employing, there was but one conclusion to which he could arrive, and that is, that we should, in all cases, employ at once the most certain and most efficacious practice, and neither waste time by delay, nor prostrate the powers of our patient by the use of other remedies, when we possess one so incomparably their superior. He believed that by the use of chloroform the ends which we have been in the habit of seeking by other means can be more speedily attained, and with greater certainty; that delay is not experienced, and durable depressing influences are not excited; and that in the majority of cases of strangulated hernia, the administration of chloroform should be primarily selected.

In quite recent hernia he believed that the warm bath and full doses of opium, followed by the gentle application of the taxis, may be expected to succeed, and it was in such, and in such alone, that he

advised their use. But he added that such cases in hospital practice were unfortunately rare, and it was too true that four, five, six, seven, and even ten days, were the average periods of strangulation of the cases admitted into Guy's Hospital.

The author stated, that when the patient had been completely brought under the influence of chloroform, the force which is required to reduce the hernia was simply nominal; anything like violence must prove injurious, and would be unwarrantable. The most moderate application of manual pressure was sufficient if success was to be obtained; and if it should fail, no extra force will be more beneficial: if success is to follow its application, moderation will suffice, and if failure, force will not succeed. If failure ensues, herniotomy should be at once performed; no other means are left from which to select. The most simple and efficient means have been employed, and have failed, and an operation alone remains.

Mr. Bryant then dwelt on the evils of prolonged or forcible taxis, and of delay, as caused by the use of other means, and stated that he believed it would be better for all cases of strangulated hernia to be treated at once by the administration of chloroform, followed by the gentle taxis; and on that failing, by herniotomy, than that any risk of delay should be occasioned by the use of remedies, or of injury to the strangulated bowel by the continued and repeated applications of the taxis. He believed that by such a practice the cases of death from strangulated hernia would be but rare, and the cases of maltreatment would be less numerous.—*Lancet, March* 30, 1861, *p.* 316.

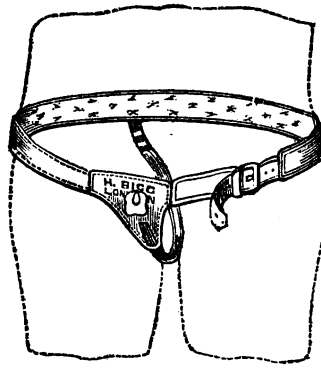
51.—NEW TRUSS FOR HERNIA.

[There are many undoubted disadvantages arising from the use of a common steel spring truss. Hence many attempts have been made at producing a bandage which shall fulfil the required mechanical conditions, without involving the necessity for a metallic band.]

Every surgeon is aware that in reducing a rupture, force has to be employed in an upward, backward, and outward direction; it is therefore evident that any appliance which fulfils the same office as the hand of the surgeon should have these three directions of mechanical force so combined, that their resultant follows the same line as the hernia itself pursues in descending. These desiderata are accomplished by a truss which has been recently invented by Mr. Heather Bigg, a diagram of which will explain its external form.

The truss has been called a "Triple Lever Truss," on account of there being three small levers, having their anterior extremities concealed within a triangular pad, accurately fitted to the inguinal region of the abdomen. These levers are acted upon by turning a little button in the middle of the pad, when the pressure of the truss can be

regulated in such a manner as to offer the exact amount of resistance required to support the hernia.



The advantage of this arrangement is highly important both to the surgeon and the patient, as it enables the pressure to be determined with scientific exactitude, and moreover admits of the same truss being at once adjusted either for violent exercise or the slightest exertion. It thus presents the peculiar condition of the same truss being equally applicable for the slightest as well as the most severe cases of hernia.

It should also be stated that, instead of a steel spring surrounding the body, a soft padded band is employed, so that the truss can be worn either at night or day, and under no circumstances whatever become in the least degree displaced. The levers not being rigidly connected are always in a state of equipoise with regard to each other, so that the pressure of the pad remains uninfluenced by any movement of the body.—*Lancet, Dec.* 1, 1860, *p.* 542.

52.—*Strangulated Hernia: A New Operation.* By Dr. JOHN NIVEN, Edinburgh.—[The writer considers the operation proposed most likely to be useful in the case of small and recent protrusions where there is little chance of adhesions having been formed.]

Every surgeon must have experienced the difficulty of reducing such herniæ by the taxis, and must also be aware of the exceeding ease with which a force acting from within, such as the peristaltic action of the bowels, effects their return. Many years ago I had occasion to perform the post-mortem examination of the body of a female, who died from an unreduced femoral hernia of this character, and was astonished to find that the slightest touch of the finger

caused the protruded bowel to slip into its place before I had an opportunity of demonstrating it to the bystanders.

The operation I propose is this:—An incision is to be made in a vertical direction, about an inch or an inch and a half above the neck of the sac. dividing the skin of the abdomen, and gradually diminishing in extent till the peritoneum is reached. The peritoneum is then to be opened to the extent of admitting a blunt hook, or, what is perhaps better, a fine curved pair of forceps, with which the neck of the protruded intestine is to be grasped, and gently drawn upwards, whilst the taxis is applied from the outside to assist. The wound is then to be closed with a harelip pin or a wire suture.

Should this proceeding fail from any cause to effect reduction, it is open to the surgeon either to enlarge the wound, so as to admit the finger and to divide the stricture from the inside, or to perform the usual operation.

The advantages of this operation are the small extent of the incision into the peritoneal cavity, and that in a sound place; and the avoidance of the exposure of the protruded bowel to the fingering required in the usual method. In fact, the danger would not be greater than the operation of paracentesis.

[Dr. Niven forgets that the relaxation of the muscular fibres after death would account for the easy reduction of the gut in the post-mortem—the same would not be the case during life. Moreover, the handling of the gut with forceps is a proceeding not without much risk.—Eds.]—*Lancet, March* 16, 1861, *p.* 276.

53.—*A Case of Strangulated Oblique Inguinal Hernia treated by inverting the Patient.* By Dr. H. Power.—[The patient who was 44 years of age had suffered from hernia for twenty years, for which he had always worn a truss. The hernia came down in spite of the truss during a violent exertion, and resisted all his own efforts at reduction.]

On examination a very tense tumour was found in the right inguinal region. It was about equal in size to a guinea-fowl's egg, and was extremely tender to the touch. I could not ascertain whether the hernia was direct or oblique. He complained of nausea, and of pain radiating over the whole abdomen.

I placed him on his back, with the knees drawn up, and for five minutes endeavoured to reduce the hernia by steady pressure, but no impression whatever was made upon it. Recollecting the plan which was rediscovered or reintroduced by my friend Mr. Jessop, of Cheltenham, and of which several successful instances are on record, I obtained the assistance of one or two of the students and placed the patient on his head. On again gently compressing the tumour, I had the satisfaction of feeling it quickly recede, and in less than a minute it entirely returned with an audible gurgle.—*Lancet, March* 23, 1861, *p.* 287.

54.—*On the Removal of Hemorrhoids by the Ecraseur.*—The first surgeon in Paris who drew attention to the occasional occurrence of traumatic stricture of the rectum, as a consequence of the use of the écraseur, was M. Nélaton, of the Hôpital de la Clinique. This practitioner, in a clinical lecture last spring, noticed this drawback to the use of M. Chassaignac's favourite instrument, and said that many of the patients who had been operated on had had reason to regret the exchange of a bearable complaint for a veritable torment. M. Chassaignac, who is not the man to abandon a cause he had once declared a good one, at once set about a modification of his original plan, and now, instead of including the whole mass of the hemorrhoidal tumor within the loop of his instrument, and removing it as if it were a polypoid growth, he contents himself with the excision of the crown of the tumor, trusting to the subsequent inflammation and cicatricial contraction to effect the rest of the process of obliteration requisite for the accomplishment of the cure. This new method, however, does not give the patient the benefit of one of the great advantages attached to the use of the écraseur under ordinary circumstances—namely, the perfect immunity from hemorrhage, as proved by the occurrence at the Hôpital Lariboisiére of a death from the above cause after an operation for the extirpation of piles, although, as very justly observes one of the leading journals here, the Journal of Practical Medicine and Surgery, it has yet to be proved whether in the statistical reports of such operations, either by écraseur or other method, fatal terminations be not occasionally observed also. M. Chassaignac's error has hitherto undoubtedly consisted in too generous an excision of healthy tissue—a proceeding which has been followed by the formation of a large and progressively contracting cicatrix. The alarm has now been pretty widely given in France at least, and operators will for the future be more economical in their dealings with the mucous membrane of the rectum. If M. Chassaignac possess any imitators in England, it will be well for them to take warning.—*Lancet Correspondent.*—*Dublin Hospital Gazette, Dec.* 15, 1860, *p.* 375.

55.—ON PROLAPSE OF THE RECTUM.

By Henry Smith, Esq., F.R.C.S.

[There has been some divergence of opinion amongst surgeons, as to whether most frequently the prolapsed part consists of the mucous and muscular coats of the rectum, or of the mucous coat only. Mr. Smith, from a careful examination of the parts, both during life and after death, considers that in the majority of cases the mucous membrane simply is implicated, but that, in a few, the muscular coat likewise is protruded. The treatment suitable to the former will not cure the latter. The health of the child, and especially the power of the muscular system, must be improved by the employment of nutritious diet and powerful tonics.]

The other instances in which we find that there is protrusion of the entire structure of the bowel are seen in those cases of long-standing prolapsus in adults, where the protrusion is of immense size, as large as the fist or a fœtal head, and coming down on the least exertion of the patient. Within the last week, I have been consulted on a case of this description, occurring in the person of an old gentleman, of a weak frame and feeble circulation. The prolapsus had existed for forty years, and it has reached its present enormous size in consequence of neglect of proper surgical treatment. In such cases as this, there is no doubt that, at first, the mucous membrane of the bowel was simply protruded, but afterwards the other tissues became involved, and at length the tumour consisted not only of the thickened mucous membrane, but the muscular coat also was extended beyond the sphincter.

In those cases of prolapsus of the rectum of much more frequent occurrence, where the disease is more limited in extent, and where the mucous membrane alone is protruded, there is a considerable difference in the pathological features, and that too of considerable practical importance, especially when viewed in relation to a mode of practice I am in the habit of adopting. In some of these cases it will be found that the mucous membrane is simply extended beyond the sphincter, in but a very slightly altered condition, the whole circumference of the lining membrane of the bowel may be down, or only one or two semicircular folds may be prolapsed: in other instances, however, besides this prolapsed membrane, the protruded part may consist of the muco-cutaneous lining of the sphincter, in a highly congested and thickened condition, forming in fact, the greater portion of the disease. This part is protruded first, and may be seen as a dark blue ring around the anus, while situated above it is the proper mucous tissue of the bowel simply relaxed and prolapsed, but otherwise in a normal state.

There is a point of considerable importance, both pathological and practical, in connexion with prolapsus, which has not been sufficiently alluded to by writers, this is the condition of the sphincter ani. In some cases we shall find that this muscle acts in a normal manner, and that the anal aperture is not larger than ordinary, although there may be a considerable prolapsus of the mucous membrane; in other instances the sphincter seems to have lost a considerable degree of its contractile power, the aperture is enlarged and easily distended; in a few cases to such an extent that the whole fingers, when formed into a cone, may be passed into the rectum. In these instances this laxity of the sphincter is the chief cause of misery, for when it exists in a great degree the patient loses, either partially or entirely, control over his rectum, and the fæces escape involuntarily. A remarkable instance of this fell under my care in the person of a patient, aged 70, who had suffered for twenty years with prolapsus, and indeed it was this circumstance which drove him to consult me. When there is a

partial loss of the power of the sphincter the patient is continually harrassed by calls to the closet night and day, although there may not be any actual involuntary discharge of fæces. Of course this loss of power of the sphincter is the greater misfortune of the two, but in some instances of prolapsus of the mucous membrane when the sphincter is in a healthy condition, the following accident may and does occur, especially when the protruded membrane has on its surface one or more distinct hemorrhoidal tumours, the protrusion occurs on one occasion to a larger extent and the patient cannot return it as usual, the most severe symptoms of course rapidly set in, and although this accident is very likely to be followed by a cure in consequence of sloughing of the constricted parts, yet one would be very unwilling to bring about this condition purposely, for death has followed upon the intense amount of inflammation which has occurred. Not long since I was called to an old lady, 70 years of age, to whom this accident happened, and not being in a healthy condition she was reduced to a great amount of suffering, for violent inflammation and sloughing of the protruded membrane had taken place; this latter process was hastened by placing ligatures around the diseased parts, and she made a good recovery.

The treatment which should be adopted in cases of prolapsus of the rectum must differ according to the pathological condition of the part, especially as regards size and the state of the sphincter. It has hitherto been customary among surgeons to use the ligature in most of the cases of prolapsus requiring surgical operation, and of these I am only now talking; and undoubtedly, where the disease has become very extensive, and particularly when associated with distinct hemorrhoidal tumours, the ligature must be used, if there be not any contraindication to a surgical operation. This is especially the case when, from the large size and the peculiar feel of the tumour, there is every reason to believe that the muscular coat of the bowel is protruded as well, for any operation short of the ligature will be useless in removing the disorder. In some of the cases also alluded to, when the prolapsus is voluminous, and there is a very relaxed state of the sphincter, the ligature alone can be depended upon; but for such instances, which are by no means uncommonly met with in old people of the middle and upper classes, I have lately put in practice an operation which I do not wish to claim as particularly new, for it is a combination of two agencies employed before for similar conditions, but which I particularly wish to bring before your notice. It consists in first applying the strong nitric acid, on one or more occasions, to the mucous membrane; and subsequently when this agent has had some decided effect, to remove with curved scissors narrow strips of skin and mucous membrane from around the verge of the anus at right angles to the orifice. The latter remedy alone was employed both by Hey and Dupuytren, and lately recommended by Syme; but my experience tells me that alone it is not to be depended upon; but if

the mucous membrane, which is always in such cases in an extravascular and relaxed condition, is first brought into a more healthy state by the contracting and slightly escharotic powers of an agent like nitric acid, the effect of removing the loose folds of skin which are so generally associated with the relaxed state of the sphincter, is very admirable. Two objects, in such instances, are sought by the surgeons, and indeed are absolutely necessary for an efficient remedy —viz. the contraction of the mucous membrane, and the bracing up of the sphincter: these two results are brought about by the combined proceedings mentioned. I must, however, not omit to state that it is perfectly useless to employ the nitric acid in those instances where the prolapsed mucous membrane has become thickened and indurated, as is very often the case: the agent will produce no good effect; both patient and surgeon will be disappointed. The application of the acid is more especially advisable in those cases where the mucous membrane is granular, very vascular, and readily bleeds: the effect of one application in such an instance is sometimes really astonishing. But there is one caution I wish to impress; and that is, that the surgeon must not be misled into abandonment of further measures because after one application of the nitric acid he finds that the bleeding and prolapsed bowel suddenly disappears. It will sometimes happen that one application will be followed by remarkably good results like these, and that afterwards the symptoms return. It is better, in cases of extensive prolapsus, that the acid should act gradually than suddenly; the effect will be more permanent. It will be necessary to apply the nitric acid when the disease is extensive, on several occasions, perhaps four, six, or eight times; but it is generally attended with so little pain, that the patient does not object to submit to it. If the sphincter ani be not in a weak condition, but acts normally, there may not be any necessity of cutting away the thin slips of muco-cutaneous covering: but if there are any pendulous flaps of integument, these should be excised. These operations, which should be effected by sharp curved scissors, are of course painful; but the application of the freezing mixture of ice and salt will much deaden the pain.

This treatment may be considered as perfectly free from danger, and is admirably adapted for those cases where the patient will either not submit to the ligature, or where there is some contra-indication to this proceeding. For instance, many of the worst cases of prolapsus occur in aged people who are, or who consider themselves, too old to undergo the ligature: others have some symptoms of lurking organic disease about their brain or heart, and it would be highly perilous to use the ligature, but the treatment I advocate may be used with perfect assurance of safety. To illustrate this important fact I will allude to two cases which have lately been under my notice. The first was a gentleman, aged 73. He had a bad prolapsus, and when told by me that I could only recommend the ligature with confidence, to destroy

his disease, he refused to undergo it. I tried by some applications of nitric acid to remedy it, but he became dissatisfied and consulted a surgeon of great eminence who strongly recommended the ligature and applied it; the patient died three or four days after from an attack of apoplexy. Now there were certain indications about this gentleman which would have prevented a surgeon who knew them and carefully considered them, as I had done, from frequent observation of his case, from performing this operation. The patient was very peculiar in his manner and habits, and thought by his friends to be very "strange," as the term goes, and in addition to this he had almost entirely lost the control over his bladder during the last few months of his life, without the existence of any stricture or disease of the prostate. Now these two facts indicated some lurking mischief about the nervous system, and should have prevented the surgeon from operating. No doubt the stimulus of the operation of the ligature which is much more severe than is imagined, lit up the lurking mischief in his nervous system and destroyed life. To this the eminent surgeon who performed the operation readily assented when he was questioned by me.

The other instance is that of an old military man, nearly 70, who has had prolapsus with severe pain and bleeding. He was anxious to get some relief; he had a peculiar nervous twitching about his face, and a feebleness of the lower limbs; and on making inquiry of one of his family, I ascertained that he had had something approaching a fit on two occasions. I at once decided against employing the ligature, for this reason, and resorted to the employment of the treatment I have advised with great benefit.

It is not to be supposed from the remarks I have made that I am averse to the ligature in suitable cases, but if it can be dispensed with, and a milder mode of treatment can be successfully adopted, the surgeon is bound to put it in force. For although I believe, when properly performed and in healthy subjects, the operation of ligaturing portions of the mucous membrane of the rectum is by no means dangerous, still we cannot conscientiously tell a patient there is no risk; independent of the peculiar danger attending the proceeding, such as *pyœmia* or *tetanus*, some serious and annoying accidents are liable to follow this operation, and I shall here draw attention to some of these.

One very peculiar and unlooked-for sequence of this operation for prolapsus worthy of relation occurred in practice not long since. I operated upon a fat old lady who had not much stamina,—the circumstances of the case were such as to demand a speedy and efficient operation; I used the ordinary precautions in the process, and the patient did very well the first day or two. On the third day, however, to the great surprise of her medical attendant and myself a severe hemorrhage suddenly took place from the part, and had such an effect on the patient that it made me very anxious. I was at a loss to account for this very unusual occurrence, where neither the knife or

scissors had been used; but on going to make an examination I found that there had been a rapid slough as large as a shilling formed by the side of the rectum laying bare the muscular coat of the bowel for near an inch in extent, and no doubt one of the inferior hemorrhoidal arteries had been opened up and hence the bleeding which was so profuse and which occurred on a second occasion; but fortunately by keeping up pressure, and by the local application of strong nitric acid, the sloughing process was stopped and the patient made a good recovery. But this might have destroyed the patient had it gone on further. The cause of the sloughing was this:—The patient, who was very fat and heavy, lay a great deal on her back after the operation, and the pressure of the bed induced the sloughing of the part already of necessity irritated by the close proximity to the ligature. We learn from this interesting case the importance of not allowing patients to lie much upon the back after this operation, they should rather be induced to lie on either side.

There is another point connected with the operation of the ligature of the mucous membrane of the rectum which calls for attention, and about which there is considerable divergence of opinion. I refer to the supposed danger of suddenly arresting the great discharges which are so frequently seen attending on these complaints. I believe that, in some instances, the sudden arrest of bleeding, or of a very profuse muco-purulent discharge by the ligature, is liable to be followed by fatal consequences, although many erroneous notions have been promulgated about this. In very robust or full-blooded persons, the sudden stoppage of the discharges may predispose to, or bring on, some internal congestions of the brain or lungs; and, therefore, in such, one should be careful about employing the ligature. I have never had good cause, in my own practice, to suppose that any serious result of this kind has occurred, but I will briefly detail one case which, if the circumstances had not been accurately investigated, would have led one to the supposition that the operation produced a disastrous result of this kind.

In March last one of the finest-looking officers in the army, of large frame, in robust, ruddy health, and aged fifty, consulted me for prolapsus and hemorrhoids of a severe nature, which had existed for many years. He was a cavalry officer, and had seen arduous service in various campaigns, having to ride much; and for many years, on such occasions, the hemorrhage was very profuse; but—*and this is important*—there had been scarcely any bleeding for the last two years. I advised the ligature, which operation was also strongly recommended by Mr. Fergusson, who, at my request, was consulted. I performed the operation, which was followed by no untoward symptoms, and in the course of seven or eight days, the ligatures had separated, and I left the patient under the care of his medical attendant, Mr. Coleman, of Kingston. The patient was out in a fortnight, but did not regain his strength so readily as is the case after this operation; and one night, about three weeks afterwards, he suddenly dropped down dead. Now,

at first sight, one would be inclined to say that here was the very case to prove the danger of suddenly stopping the discharge from the rectum. And had I not investigated the point, I should certainly have supposed that a sudden congestion of the brain had been caused by the cessation of the discharge; but the fact of there having been scarcely any bleeding during the last two years, militates against the doctrine. Mr. Coleman, who is a very able and shrewd practitioner, considered that it was disease of the heart that destroyed life,—that the man had a feeble heart, and that the low diet to which he had been of necessity reduced, had further enfeebled it, and hence its action had suddenly ceased. It was a most unfortunate thing that no post mortem examination could be obtained.—*Med. Times and Gazette, Nov.* 17, 1860, *p.* 475.

56.—*Salivary Fistula.* By R. G. H. Butcher, Esq., Dublin.—[The following short abstract shows well the mode of dealing with cases of salivary fistula. The case was one of removal of the whole upper jaw and malar bone, for the extirpation of an enormous tumour.]

On the 7th April, I opened a small abscess in the parotid region, or rather in front, over the masseter muscle. From this flowed out pus and saliva in abundance, the latter fluid continuing for some days to drain freely from it. I passed an armed probe with a few threads of silk from the external opening, through the abscess, forwards into the mouth, and tied the seton up; and in a few days, having established a channel for the saliva into the mouth, I pared the edges of the external wound, brought them together, maintained them so by two points of twisted suture, using very fine needles, and in three days the wound was healed and closed permanently, and the saliva continued to pass by the artificial canal into the mouth.—*Dublin Quarterly Journal, Feb.* 1861, *p.* 11.

57.—*Artificial Teeth.*—Coralline (coloured vulcanized India-rubber) being now extensively employed as the basis of artificial teeth, it requires to be satisfactorily determined whether the subsulphate of mercury used for the colouring is likely in any case to produce prejudicial effects. The material being very hard and insoluble, and not capable of being much softened even in boiling water (the dentists employ steam of 230°), there seems no danger from the vermillion pigment; yet as the extent of surface in a full set of teeth is twelve or fourteen, or more, square inches, and the material, at the temperature of the human body, is acted upon day and night without ceasing by the saliva, and the gums and tissues which come in contact with it are peculiarly susceptible of mercurial influence, it may not be quite certain, without the test of experience, that no evil can arise in the case of a morbidly sensitive invalid. If any such cases had been known

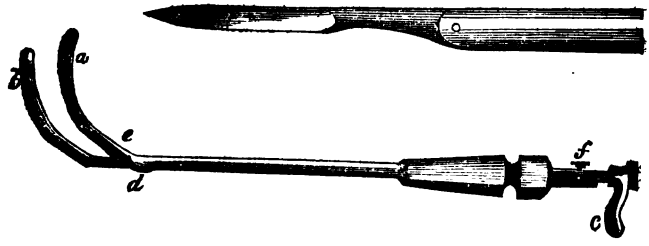
to occur, the dental profession would assuredly have changed the cinnabar for some other pigment. But the use of the material is recent, and it would be well to watch the testimony of facts as to whether any of the spmptoms of mercury (not amounting to salivation or other marked phenomenon) can be suspected in the mouth or pharynx or alimentary canal.—*Lancet, March* 23, 1861, *p.* 300.

ORGANS OF URINE AND GENERATION.

58.—LITHOTOMY.—NEW FORM OF STAFF FOR DILATING THE PROSTATIC URETHRA.

Case under the care of JOHN WOOD, Esq., King's College Hospital.

The patient was a boy, aged 9 years. The stone was small, and the contact with the sound was somewhat obscure. In the performance of the operation, Mr. Wood introduced to the notice of the class an instrument devised by him, which combined the uses of the staff and a dilator of the prostate,—a Dilating-staff. In the subjoined woodcut it is given on a greatly diminished scale. It consists of a staff



mounted with a stout handle, and opening at the commencement of the curve into two blades, which pass into the bladder and act on the prostatic urethra as dilators. The anterior blade (*a*) is immovable, and fixed at the end of the steel tube upon which the handle is placed, and which forms the shaft of the instrument. The posterior blade (*b*) is connected by a rod passing through the beforementioned tube, with a small lever (*e*) projecting to the right side of the handle, and works (on the application of the thumb of the hand holding the staff) by a double motion. One part of the motion is antero-posterior—by a piston-like movement; and the other lateral—on the axis of the connecting-rod, in such a manner as to lay the blade flat upon the base of the bladder, holding it downwards towards the perineum, and preventing it from yielding before the dilating finger. At the end of the tube whence the anterior blade springs, is a slight bulbous enlargement (*d*) capable of being felt in the perineum when

introduced and placed in the bulb of the urethra, just anterior to the deep perineal fascia. Beyond this the blades open, and for the length of an inch are straight in themselves, but inclined upon the shaft at an angle of about 30°. This part is intended to lie in the membranous urethra. The blades are then curved sharply for about three inches, so as to hook well over the pubes. On the inner surface of the straight part of the anterior blade, where opposed to its fellow, is a deep groove (*e*) an inch long, which is fully exposed by the opening of the blades. Upon this groove the membranous urethra is opened to admit the finger of the operator while it is stretched tightly by the divergence of the blades. The internal or opposed surfaces of the blades open upon each other diagonally or obliquely, and are smooth, even, and flat. Between them, when opened, the dilating finger of the operator slides through the prostate into the bladder. The pin (*f*) prevents the locking of the blades by turning, when open, in the wrong direction.

The Operation.—The patient being placed in the usual position, but without being tied, and the staff introduced. the operator first placed the bulb of the staff accurately in the bulb of the urethra, as far down as it would pass easily. The shaft of the instrument was then exactly at right angles to the axis of the body. The assistant then opened the blades by pressing the lever from him with his thumb. The operator next placed the left forefinger upon the projection of the staff at the bulb, and the thumb upon the right tuber ischii—stretching the perineum, and sliding upwards, as far as possible, the superficial part of the bulb of the urethra. Then, with a long narrow lithotomy knife (drawn in the woodcut of a reduced size), with an inch and a-quarter of cutting edge, a lunated incision was made in the perineum, commencing about two lines to the right of the median raphé, just posterior to the bulb, at about one-third of the distance between it and the anus, and carried round the latter, terminating opposite its anterior margin, midway towards the tuber ischii, thus describing the quadrant of a circle, having the anus for its centre. This incision passes through the superficial fascia and central tendon at the anterior margin of the sphincter ani, without dividing the muscle, and avoiding the chief perineal vessels and nerves. The membranous urethra being thus exposed, it was then felt by the left forefinger and opened upon the cleft and groove in the staff a little to the left of the median line. The tense and widened condition of the urethra rendered this proceeding very easy and certain. The finger then entered the cleft between the blades of the staff. A slight touch with the knife upon the urethral edge of the posterior layer of the deep fascia then enabled the finger to pass fairly between the blades into the prostatic urethra, which was slowly dilated by rotation of the finger upon its axis. The prostate gave way easily, the stone being immediately reached and removed with a small scoop. A very small quantity of blood escaped from the wound.

The remarks after the operation were illustrated by a drawing of the natural size, made from a careful dissection of the perineum, and by a diagram showing the relative positions of the pelvic viscera as seen by an antero-posterior vertical section of the trunk. These had been carefully obtained by measurement of the distances and angles in forty dissected subjects, young and old, during the last ten years. Mr. Wood called particular attention to the arrangement of the posterior layer of the deep perineal fascia, its connection with the recto-vesical, obturator, and anal fasciæ and capsule of the prostate posteriorly, where it separates the urethra and its muscles from the levator ani; and with the anterior layer or triangular ligament and deep layer of superficial fascia below, where it is blended with the central tendon near the anterior margin of the anus. He attaches great surgical importance to this strong fascia, and classified all the perineal operations for stone, into—First, Those in which the incisions are made entirely anterior to it, aiming at the membranous urethra; and, 2nd, Those in which the knife is carried behind it into the ischio-rectal fossa, dividing more or less of the levator ani muscle and capsule of the prostate with its large venous plexus. The objections which he entertained towards the latter class of operations are based upon the high rate of mortality resulting from their performance: 1st. From pyæmia, the risk of which he considers to be greatly increased by opening the prostatic venous plexus, connected as it is intimately with the large vesical and hemorrhoidal plexuses; 2nd. From diffuse pelvic cellulitis and consequent peritonitis, which he believed to be mainly owing to section of the pelvic fascia above the levator ani, especially when the muscle is not at the same time divided freely so as to allow the urine a free escape. If the levator ani, or its sheath, be cut at all, it ought to be divided freely. The ureter was in some cases cut off by a free incision, over which accident the surgeon often had little control, with the point of the knife in the bladder so deep from the surface. The accident was rendered the more likely to happen by the spasmodic contraction of the muscles of the trigone approximating the ureters to the urethral orifice, directly in the oblique line of incision into the neck of the bladder, on the contact of the knife with the highly sensitive surface of the trigone. It had happened that, when the bladder was empty at the time of the operation, the point of the knife had passed through its superior wall. 3rd. From hemorrhage. This is more frequent after a free use of the knife in the prostate. In old persons it is often extensive from an enlarged and varicose venous plexus. Sometimes it proceeded from an irregular deep-seated pudic artery, and occasionally from the inferior hemorrhoidal vessels, or from the artery of the bulb.

Of the operations performed in the anterior perineal region, that which had commended itself the most was the median operation as proposed by Allarton. To this method there were some objections, which Mr. Wood considers important. In a boy aged nine years, (the

age of the patient operated on,) he had found the distance of the bulb from the anal bend of the rectum in the median line to be half-an-inch. The mean distance in adults was only an inch and a-quarter, from the greater prolongation of the bulb backwards in the median line. The bulb being closely adherent to the triangular ligament by its deep surface, it was impossible to push the whole of it upwards, as some suppose. Consequently, in the median operation, the bulb is invariably more or less cut, to get room and to avoid the rectum. It was thought by many that section of the bulb was of no great consequence; with this opinion he could not agree. Free section of the erectile tissue and venous cells of the bulb for stricture of the urethra had frequently been followed by pyæmia. If avoiding the bulb increased the chances of the patient but 1 in 100, he felt strongly that it ought to be done. Without wishing to over-estimate the dangers of bleeding from this source, he might mention that a very experienced lithotomist had told him that he had had alarming hemorrhage from the bulb in several of his cases. Mr. Wood also thought that stricture might very likely follow an extensive cicatrix in the contractile tissue of the bulb in children.

Another serious objection, was the want of room to work the forceps round the bladder, especially if the stone were large and the perineum deep. In one case difficulties of this nature had been followed by the death of the patient. The means adopted in Allarton's method were not powerful enough to overcome an indurated or tough prostate. Hence the invention of dilators and modified gorgets. In the loose tissues of the child, the slipping of the finger from between the staff and the probe would carry it directly between the bladder and rectum, and forcible attempts at entrance into the former, would even endanger the entire separation of the membranous urethra from the prostate. The like objection applied to the limited use of the knife in section of the prostate, and attempts to enlarge the opening by dilatation with the finger only upon the staff. If the median incision were necessarily or accidentally continued into the prostate, the ejaculatory ducts were sure to be cut off.

From these dangers Mr. Wood submitted that the operation he had just performed was free. The external incision gave ample room, and was capable of extension in the usual site of the lateral operation if necessary for a very large stone. It avoided the bulb, its artery, the rectum, and larger perineal vessels, and corresponded closely to the plane and lower border of the deep perineal fascia, which he felt it to be so important to avoid cutting. By opening the membranous urethra on one side, he got a greater length between the bulb and prostate in which to insert the point of the dilating finger, and avoided the danger of cutting off the ejaculatory ducts. The advantages of the dilating staff he considered were as follows:—

First. Its extending action upon the membranous urethra renders it easier to make a clean cut into the tube, and to place the point of

the finger fairly in the urethral canal. Second. It guides the finger in the prostatic channel with the almost absolute certainty of a double conductor, one on each side. Third. It aids powerfully in the dilatation, acting from above in concert with the action of the finger from below, to which it permits, at the same time, more complete tactile perception of the resisting tissues than in the use of gorgets or dilators. Fourth. It renders the passage of the finger between the pubis and bladder almost impossible, by the pressure of the anterior blade, and diminishes to a minimum the danger of getting between the bladder and rectum. Fifth. It holds the base of the bladder firmly downwards to the rectum and perineum against the dilating force from that direction, which it thus renders more effective, and entirely removes the danger of tearing off the urethra from the bladder. Sixth. It removes the necessity of an extra instrument for getting through the prostate; and lastly, there is that positive certainty of avoidingsection and exposure to the urine of the layers of the pelvic fascia, which cannot be obtained by the use of the knife or gorget in the prostate.

These reasons Mr. Wood considered strong enough to justify the introduction of another method of operating in the face of the many methods in vogue.

He concluded his remarks by pointing out the different axes of the urethra and bladder, and their relation to the central tendon, the rectum and the two planes, anterior and posterior, in which he showed the surface of the perineum to lie.

Up to the present date the patient has not had a single bad symptom. The wound is contracted to about an inch, and covered by healthy granulations. There are appearances indicating the occasional passage of the urine by the meatus externus.

[A paper was read by Mr. Wood on the above subject, before the Royal Medical and Chirurgical Society, on Jan. 8th, 1861, after which]

Mr. Fergusson expressed himself pleased with the great beauty of Mr. Wood's diagrams; but the anatomy which they taught was no novelty. It was well known and taught by anatomists that the bladder in the child was much more out of the pelvis, than in the adult; but he had not seen this part so well displayed before. He objected to the instrument invented by Mr. Wood as far too fragile in structure and too uncertain in its probable mode of action, and should prefer the old grooved staff. The prostatic and membranous portions of the urethra were often with difficulty dilated, as proved in the removal of calculi by the forceps; and, except in very young persons, Mr. Wood's instrument would fail to effect dilatation, and even in them its action could not be relied upon. He had no objection to any novelty which might be of service in simplifying or making more easy the operation of lithotomy, but this could not be done by the results of one operation, or by experiments on the dead body. Mr. Fergusson then spoke of

the importance of discussing the subject of lithotomy, which of late had not been done; whilst lithotrity had attracted much attention. Of late, there had been a strong feeling in favour of the median operation of lithotomy; but this had been performed chiefly on children, in whom the lateral operation was almost uniformly successful: to show its superiority, it should be tried in the adult. Statistics in large numbers only could decide the question; but he saw no reason, at present to prefer the mesial to the lateral operation.—*Med. Times and Gazette, Dec.* 22, 1860, *and Lancet, Jan.* 19, 1861, *pp.* 610, 62.

59.—ON SOME OF THE CONSEQUENCES OF STRICTURE AND THEIR TREATMENT.

By F. Le Gros Clark, Esq., Surgeon to St. Thomas's Hospital.

[*Case* 1.—A waterman, aged 46, was admitted August 17. He had had gonorrhœa many years since, succeeded by permanent stricture. For some years his urine had dribbled from him guttatim without any effort. Two weeks before admission a swelling appeared at the junction of the scrotum and perineum, and an abscess opened spontaneously, through which the urine found its way.]

On admission there was a hard, circumscribed, and tender swelling in the perineum, and a fistulous opening in front of it, through which the urine escaped in drops. As he lay in bed he kept a vessel by his side to catch the water, which was constantly dribbling involuntarily, from the penis: in fact he had entirely lost the power of retaining his water, and was incapable of making any effort to expel it. An attempt to pass a catheter was futile. His general health was feeble, but there was no fever, nor marked constitutional disturbance beyond this. After the lapse of three days, as the symptoms were not ameliorated, he was placed in the position for lithotomy, and a staff being passed down to the seat of stricture, a free incision was made through the back part of the scrotum and perineal swelling upon the staff, a director being employed to guide the knife along the fistulous opening: a small quantity of ill-conditioned pus was evacuated. The urethra having been sufficiently divided beyond the staff, this instrument was removed and the patient returned to bed. A nutritious diet was ordered, and a poultice to the wound.

In the course of two days the urine began to flow freely through the perineal opening, and the patient's general condition was improved; at the same time, the stream being diverted, the water dribbled less from the penis, until it nearly ceased. Some lint was subsequently kept in the perineal opening, which was disposed to granulate too freely near the surface, and some tincture of the sequichloride of iron was given, in ten minim doses, thrice a-day. At the expiration of a month the wound had nearly healed; the incontinence had ceased; and the length of time that the patient

could retain his water had gradually increased, and he was able to pass it in a tolerably free stream. There was a still a small fistulous opening in the perineum. On September 28 he left the hospital.

Case 2.—Thomas P., aged 63, a sailor, was admitted on September 21. He had been the subject of stricture for thirty-five years, and had frequently been subjected to catheterism for relief, but not lately. He stated that he had on several occasions suffered from retention of urine; and about eight months since he had an abscess at the back of the scrotum, which burst and allowed the escape of urine. Five days before the perineum began to swell, with severe throbbing pain; the scrotum was also tumid and inflamed. Urine was passed both by the perineal opening and penis, but with straining and pain. There were two or three fistulous openings in the under part of the scrotum, with indurated orifices. Feeble power, and constitutional disturbance indicated by foul tongue, quickened pulse, &c. Urethra impermeable by a small catheter. The perineal section was performed in the same way as in the former case; and some fetid pus was evacuated from the neighbourhood of the urethra. The scrotum was also incised. The stricture being divided, or rather the urethra being cut into behind it, the patient was returned to bed, without any attempt being made to pass an instrument into the bladder. Bark and ammonia with wine and a nutritious diet, were ordered.

The febrile symptoms speedily subsided, and the patient improved daily in health. The wounds became clean, and granulated healthily. For some time the urine found its way principally through the perineal opening; but as the wound closed, it passed more abundantly, and at the same time more freely, by the natural outlet. At the end of a month the water had ceased to escape by the perineum; a very small quantity found its way through the old fistulous openings in the scrotum; but the patient was able to micturate in stream from the urethra, and a good sized catheter could be passed into the bladder with facility. He said that he had not been in so good a condition for many years.

The foregoing cases are examples of a class of common occurrence; but not, therefore, the less interesting and important. Abscesses may occur external to the urethra, and without communicating with it, as in cases where inflammation is propagated, by contiguity of texture, from the urinary passage to the neighbouring areolar tissue. Such collections of matter should be opened early, or they may open spontaneously into the urethra, and thus entail extravasation of urine, and its disastrous consequences. But it is far more frequently the case, that these perineal collections of matter result from mischief which commences within the urethra itself. The rapidity with which such cases run their course varies greatly. Extravasation of urine may occur suddenly, carrying almost immediate destruction to every tissue to which it penetrates; or the consequences of extravasation may be limited, as in Case 1, to a small circumscribed spot contiguous to the

urethra. The second case illustrates an intermediate condition between the two extremes; there was old disease, but grafted on this was more recent and active mischief, accompanied by constitutional disturbance, sympathetic with the extension of the urine into the scrotum. Sudden extravasation may occur in consequence of the urethra being ruptured from external violence; but such cases belong to a different category altogether, and demand entirely different treatment. When the urine escapes suddenly through a rent in the urethra, consequent on obstruction to its passage, and the incapacity of the canal to resist the force from behind, then this irritating fluid diffuses itself rapidly through the neighbouring areolar texture, which passes rapidly into a state of gangrenous inflammation, and thus perishes. This diffusion of urine is analogous to the emphysematous extravasation of air in wounded lung, but with different results. A case of such urinary gangrene came under my care in private, at the beginning of the year. I refer to it briefly, because I have not lately had a similar hospital case, and it illustrates one or two points of importance which I wish to allude to presently. When I saw this patient the distended scrotum, skin of the penis, and perineum, were infiltrated with urine, and in an irrecoverable condition; and the right groin and contiguous tegumentary covering of the abdomen, half way up the umbilicus, was likewise the seat of urinary infiltration. Free incisions did little more than allow of the more ready separation of extensive sloughs below, but saved the abdominal skin. The perineum became a gangrenous chasm, the urethra being laid open from the back of the scrotum almost to the pubic arch, as was proved by passing a sound into this part. Fortunately this patient had a good constitution, and his stomach was kept in good humour, so that he ultimately made a good recovery. The exposed testicles and denuded penis were again enveloped in integument, and the perineal chasm finally closed, leaving, after the lapse of some months, only a small perineal fistula; and even this is now closed, and the urine is passed in an unobstructed stream. No attempt was made to keep a catheter in the bladder during the entire course of the case.

More usually, however, when time is given, nature makes provision to obviate this wholesale destruction from diffused extravasation; and this is apparently accomplished, as in the cases I have narrated, in the following way. A diseased condition of the urethral mucous lining, consequent on gonorrhœa, and entailing stricture, is usually the antecedent state and history. A small point of ulceration in the urethra, possibly at one of the numerous lacunæ, extends outwards into the submucous areolar tissue, and is followed by a minute drop of urine. Inflammatory deposit takes place around this small fistulous opening, which is thus, as it were, walled in, as an ordinary abscess frequently is. But still the irritation of the urine, combined with the *vis a tergo*, gradually forces a passage onwards, and the centre of this indurated deposit at length softens down into a circumscribed abscess, to which

the urine has access. At this stage the patient may come under our care, suffering from a hard swelling in the perineum, often conical in form with its apex directed forwards, and acutely tender; perhaps attended by complete retention of urine, or at any rate by difficult micturition. There is no fluctuation in this swelling; but on cutting through hard gristly texture, generally of considerable thickness, an abscess, containing it may be only a few drops of matter, is reached, and relief is afforded. If the case run on to a later stage, as in the two instances mentioned, the pus and urine mixed finds a way out for itself; and this may be without extension of the infiltration, as in the first case, or with it, as in the latter. Still, the treatment must be the same, viz. free incision into the seat of abscess. But by this step we reach the urethra, and thus have it in our power to relieve the cause of the existing mischief, as well as the mischief itself. Now, it is to this point that I wish particularly to direct attention; for I think this *desideratum* is best fulfilled by the non-interference plan which I adopted in the cases I have narrated: abstinence, I mean, from further interference after the abscess was freely incised, and the urethra laid open behind the point to which the staff was passed down. I have tested this plan for a long time now, and have no hesitation in recommending it for your adoption as a general rule, in preference to passing a catheter on into the bladder, even if you can accomplish this often difficult task, and leaving it there. Theoretically, the latter course seems to be the most appropriate, and I believe many surgeons regard the introduction of a catheter as an essential part of the treatment. I used to think so; but am now satisfied, and have long been so, that, as a general rule, the presence of an instrument, under these circumstances, is not only superfluous, but positively mischievous. There may, doubtless, be accidental conditions which demand it, such as stricture farther back, which is, however, vary rare; or a paralytic and distended state of the bladder; but, even with dribbling incontinence it is not essential, as case 1 proves. If the urine do not flow readily at once, it very soon finds its way through the artificial opening; and we may then trust safely to nature's modelling power, as we do, for instance, in lithotomy, without supplying a mould for the new material to be modelled upon. This reparative power was well illustrated in the case of extensive sloughing after extravasation to which I have referred. But, beyond this negative recommendation, you save the patient much inconvenience, if not suffering: and I have repeatedly seen the presence of a catheter create so much irritation, that healthy action has been retarded, and even arrested, and its removal thereby imperatively demanded. As the case advances towards a cure, then an instrument either bougie or catheter—I prefer the former—may be introduced from time to time, to assist in enlarging the calibre of the urethra; but it need not be left in.

You will not, however, understand me as advocating the treatment of burst urethra from violence in this way. In such cases it should

be the surgeon's first duty to introduce an instrument, and to fix it in the bladder. This can generally be accomplished by careful and patient manipulation, and of course without an external incision, unless the mischief resulting from infiltration of urine require it. Indeed, it is the surgeon's business to anticipate such mischief; and to pass a catheter, where he suspects the urethra is ruptured, before he allows the patient to attempt to micturate unaided. A case in point occurred under my care a few months since, and I commented upon it in a former lecture—(see Medical Times and Gazette, July 14, 1860). A similar case came under the notice of several of you at an earlier period, in which a railway-guard was struck by the buffer of an engine, and had the ramus of his ischium fractured, and his urethra torn; both of these patients recovered without extravasation, from careful attention to this precaution; and thus, by allowing the urine to drain off through the instrument, so as to prevent the bladder from becoming distended, a ruptured urethra is not necessarily followed by extravasation.

One word in conclusion respecting the cases I have narrated. In both there was mischief of long standing. In the latter the symptoms were more urgent, because the infiltration was extending more rapidly. In neither was retention of urine complete, had it been so, and if the simple operation performed had not secured relief, it would then have been necessary to pass an instrument into the bladder, if practicable, or to have relieved the distended organ in some other way, as circumstances might dictate. But I believe it will be found that, even with retention, the operation I have described will frequently, if not generally, prove sufficient to give the necessary relief, if not at the time, at any rate soon afterwards. The sesquichloride of iron was prescribed in case 1, to aid the paralysed bladder in recovering its tone.

If it be asked how the division of the urethra through and behind the stricture relieves that diseased contraction permanently, I apprehend the explanation must be found in the fact that rest alone, by diversion of the stream, assists in this desirable result, as demonstrated by the operation of tapping the bladder, for retention, by the rectum. But further, I believe that in the suppurative action that follows, the adventitious tissue which causes the contraction or stricture is softened down and got rid of, and thus a more healthy and natural condition of the submucous tissue remains.—*Med. Times and Gazette, Dec.* 15, 1860, *p.* 575.

60.—ON A NEW AND UNIFORM GUAGE FOR CATHETERS.

By Henry Smith, Esq., Surgeon to the Westminster General Dispensary.

In a paper which I lately read before the Medical Society of London, relating to the treatment of stricture of the urethra, I referred to the importance of carrying on dilatation of the canal slowly and

gradually, so that as little disturbance as possible should be produced either in the part operated on, or in the system generally. I also stated that this gradual dilatation would be less liable to be followed by the return of a stricture than if any of the severe methods now often adopted were put in force, but that this kind of dilatation could not be effected in the manner it ought to be, by the instruments generally found in the hands of surgeons, in consequence of the gauges, according to which they were constructed, not being sufficiently minute in their increments, and their being no real uniformity in the scale.

My attention has long been directed to this point, and it has appeared to me most desirable that the differences in size between our catheters should be more minute than they are, and that the increase should be uniform. My friend Mr. Bishop was kind enough to assist me in this matter, and after having carefully examined a gauge according to which catheters are usually made in London, pronounced it to be both "variable and arbitrary" as regards differences in size. It was proposed that in the new guage the increments should be exactly the one-hundredth part of an inch throughout the entire scale, being very much less than in that examined; but on calculation it was found that if the unit (or No. 1) catheter were to be, as I proposed, much about the same size as that now in use, twenty or more instruments would be required before a size equal to an ordinary No. 12 could be reached. It was then proposed that the increments should be the seventy-fifth part of an inch in the scale throughout. Mr. Matthews, of Portugal Street, has made a gauge accordingly, and has constructed for me a new set of catheters, the increase in size being exactly the seventy-fifth part of an inch from No. 1 to No. 15 in each instrument. My No. 15 is about equal in size to the ordinary No. 12, therefore in the set there is an addition of only three numbers. According to this plan, the increase in size in each catheter is not only minute and exact, but it is regular throughout the series, the great desideratum hitherto wanting. Consequently dilatation can be carried on in a mathematically gradual manner,—so slowly, so painlessly, and yet so effectually, that the advantages attending this mode of proceeding must be obvious to all who reflect on the sensitive nature of the urethral canal, and appreciate the difficulty which exists in getting rid of the thickening of a permanent stricture by ordinary dilatation.

All practical surgeons who have had much to do with severe cases of stricture, have found that an instance is not unfrequently met with where a No. 2 or 3 catheter can be introduced; but where no progress can be made beyond one or two sizes; a No. 4 or 5 is arrived at, then the dilatation is stopped; either caustics. cutting, or forcible ramming down of instruments are employed; or it is only after a lapse of some time, and after the painful introduction of the last-sized catheter, on many occasions, that a larger instrument can be put into the bladder. This difficulty is accounted for by the fact of the increments in the old

gauges being, as Mr. Bishop says, "variable and arbitrary," and also by a circumstance not hitherto, so far as I am aware of, sufficiently appreciated by surgeons, viz., that in these more severe strictures the urethral canal loses its capacity for dilatation in proportion as it is dilated. It is too often supposed that because a No. 5 or 6 catheter can be passed through a stricture, it must of necessity follow that the diseased canal can be dilated to its normal state as readily as it has been dilated so far. This is a great mistake; and it will in truth be found that the further dilatation will be a most difficult task, in some cases an impossibility. This is owing, in a measure, not only to the incapacity of the canal to dilate, but also in a great degree to the arbitrary and abrupt increase in size in the catheters as ordinarily used, especially in the higher numbers. A great deal of this difficulty will be done away with by using instruments constructed according to the scale proposed in this notice, the increments in which are minute and uniform. I find great advantages in the use of these instruments, and I would strongly recommend all those surgeons who are about to have catheters made, to get them constructed strictly according to the gauge now used by Mr. Matthews.—*Med. Times and Gazette, Feb.* 23, 1861, *p.* 195.

61.—ON THE USE OF POTASSA FUSA IN THE TREATMENT OF STRICTURE OF THE URETHRA.

By Campbell de Morgan, Esq., Surgeon to the Middlesex Hospital.

[The author, in the commencement of his paper, states his belief that this mode of treatment has not received that attention which it merits. The case related is that of a tailor, aged 44, whose lungs contained tubercular deposits.]

About two years ago, he found that he could not at all times retain his urine. It passed from him involuntarily and spasmodically immediately on his feeling a call to micturate. At the same time the stream began to diminish; and it continued to do so until about three months ago, when he found that he could pass urine only in drops. From the first, he had severe and forcing pain in passing his urine. The desire to do so was almost constant; and for some weeks his rest has been completely broken by it. In the course of the twenty-four hours, he voided an average quantity, which was of specific gravity 1015; was slightly acid; and contained no albumen; and no more than the usual amount of mucus. There was no hardness nor tenderness in the perineum. On passing a bougie into the urethra, a stricture was found near the membranous part. Attempts were made to introduce the finest instruments into the stricture, but without success. He did not experience much pain from the contact of the instruments; nor did any bleeding take place. The stricture seemed very hard and resisting.

On the 6th, a piece of potassa fusa, of about the size of a large pin's head, was passed down to the stricture by means of a large bougie hollowed at the extremity to receive it. This gave rise to considerable pain, especially when urine passed. This pain, save when he tried to urinate, ceased by the following morning. For nearly forty-eight hours there was no difference in the way in which the urine flowed; it came in drops; but then he began to pass a small stream, ending in drops, and with still some scalding.

On the 10th, the potassa fusa was again applied in the same manner. He had pain as before; but soon afterwards the stream enlarged, and he began to retain his urine for a longer period.

On the 17th, a No. 2 catheter was passed readily into the bladder; and then catheters were introduced one after another till a No. 8 was passed. The urine now came away always in a stream, without any tendency to its passing in drops, though he still had some pain. He now got rest at night, and said that he was in every way more comfortable, except from his cough, which was more harrassing.

On the 27th, a middle sized catheter was again passed, but it was obstructed at the upper part of the stricture, and it required to be depressed, in order to guide it through. A small piece of the potassa fusa was therefore again applied; the hollow for its reception being made in the upper part of the bougie, so as to touch the upper margin of the stricture. The pain was the same as before, and there was some scalding for three or four days, after which he had no pain whatever. From the time of this last application no further interference was called for. The stream became free, and from day to day he had less trouble, so that after about ten days he passed a larger stream than he had done for two years, and could hold his urine for many hours; he was indeed scarcely disturbed at night. Every day he seemed to be gaining ground, so far as the urinary organs were concerned, up to the time of his death, which took place on February 1st, the symptoms of phthisis having made rapid progress during the intensely cold weather.

The caustic potash is a safe as well as a rapid agent in removing a stricture. The stricture was extremely small; I could not get a fine instrument into it; and perhaps lengthened and repeated trials would have been necessary before I could have succeeded in doing so. The attempt would probably have caused irritation, and might thus have complicated the case; for, unless a bougie pass the stricture, it is often productive of irritation. Had a bougie been passed, there is no doubt that dilatation might have been easily effected. The stricture was limited to a small segment of the membranous part, and there is no hardness to be felt in the perineum. I was satisfied, however, that not only a more rapid, but a more permanent relief, would be obtained by the application of the caustic, than by any mode of using the bougie alone; while any more decisive measures were entirely uncalled for, and would have been improper.

The patient had all the indications of phthisis; and on that account, again, it was perhaps desirable to free him from his trouble in the urinary organs as promptly and with as little irritation as possible. The case can hardly be regarded as one which shows most strikingly the effects of the treatment; but you have not perhaps seen the caustic applied, and it is worth bringing to your notice in connexion with the general question of the treatment of stricture.

The treatment of stricture by caustics, in one form or another, is of long standing. In this country the nitrate of silver was brought into general notice as a means of destroying stricture by Sir E. Home, who had seen it advantageously used by Mr. Hunter, There is no doubt that many cases of obstinate stricture were cured by it; but its use has become restricted to those in which there is a very irritable state of the membrane; and in such cases the lunar caustic acts as it does when applied to any other irritable surface; it deadens the sensibility. Its escharotic power is not, however, very decided. You have often seen it applied freely even to the conjunctiva, without its destroying the surface of the membrane. When it does act as an escharotic, it gives rise to swelling and inflammation of the surrounding parts. Its use as a destructive agent on the diseased tissues has been generally abandoned.

Mr. Whately first suggested the application of potassa fusa as a means of removing stricture. He only allowed of its use, however, in those cases where a fine bougie could be passed fairly through the stricture. Where this could not be done, he thought that the application of the caustic might set up inflammation, and so, by diminishing still more the aperture, cause a complete retention. The small quantity of the caustic which he used, and the precautions which he thought it desirable to adopt in order to prevent its too free action, made it in his hands a far less effective agent than it has since proved to be.

It is to Mr. Wade that we are indebted for a more just appreciation of the value of this remedy, and I would strongly recommend you to read his views on the subject. He pointed out the true distinction between the action of lunar caustic and that of the caustic alkali. The lunar caustic, he observes, gives rise to a hard slough, and to the effusion of lymph around it; while the potassa fusa effects a softening and dissolution of the thickened tissue, without producing any surrounding hardness. There is little risk, therefore, of causing that blocking up of the stricture which was feared by Mr. Whately. Mr. Wade also uses the caustic in larger quantity, and does not cover it with lard, as was recommended by that gentleman. He applies it to cases of impermeable stricture, whether those of merely a small cord-like constriction, or where the canal is indurated to a considerable extent; and he expresses his belief that, in cases so treated, there is but small disposition to a return of the disease. The last mentioned point is one of great importance, and if the opinion be correct, would,

cæteris paribus, render this mode of treatment preferable to most others.

As a general rule, if an instrument can be passed through a stricture, the cure is half effected. The careful gradual use of the bougie will cause the dilatation of the stricture; and, where no untoward symptom is present, such as great irritability or tendency to spasm, this is undoubtedly the best and safest mode of dealing with the complaint. But, in the majority of such cases, the bougie must be regularly used from time to time after the stricture has been dilated; for, when once a stricture has formed, there is usually a great disposition to its return. This is, no doubt, often the result of constitutional causes; the gouty constitution, or chronic dyspepsia, will keep it up. So, too, will local causes—a persistent irritable state of the bladder, or hard horse-exercise. Very frequently, however, the patient cannot give up the time required for this gradual dilatation; and frequently also the stricture will not yield to this plan of treatment. In other cases, again, the urethra seems to resent the contact of the instrument; irritation is set up whenever it is used, or the tendency to spasm is increased after it is withdrawn. In all such cases, the surgeon must resort to other modes of dealing with the stricture. Many plans of treatment have been adopted, suitable to the various complications which may exist, or to the necessities of the patient. Forced catheterism; the dilators of M. Perrève, which have been introduced into this country by Mr. Holt, and which I have used in some cases with great benefit; the dilators of Mr. Wakley; perineal section, or intra-urethral section, and other methods,—have been put in practice with more or less success; the constitutional condition of the patient being, of course, attended to. But the disposition to return is perhaps even greater in these cases than in the more simple states of the disease. Does the removal of the stricture by potassa fusa give a better chance of permanent relief? I believe it does; and, irrespective of the cases reported by Mr Wade, which are entirely satisfactory, my belief is founded partly on theoretical grounds, partly on my experience.

I have often pointed out to you the peculiar appearance of the cicatrix left after the healing of a deep issue made by the caustic potash. Instead of being puckered and hard, as you generally see in the cicatrices which follow the total or partial destruction of the skin by the application of the actual cautery or by most escharotics, it is soft and smooth, and glistening, as large as the portion of the skin originally destroyed, and merging almost insensibly into the sound tissue. My friend, Mr. Hoffman of Margate, some years ago told me of the benefit which may be obtained by destroying the unsightly puckered scars which are left after the suppuration of scrofulous glands of the neck, with this caustic. When the sore produced by the falling of the eschar has healed, you find that you have substituted a smooth even surface for the previous seamed and corded one. In the course of time, the new tissue regains so natural an appearance

as to escape notice. This plan I have often put in practice, and with excellent results. The same effect is, I believe, produced in the urethra when the diseased hardened tissue has been thus removed: a smooth surface is left, with little or no disposition to contract; and a more permanent cure is effected than is usually produced by the bougie. The *post mortem* examination allowed us, in the present case, to see the condition of the parts recently acted on by the caustic; and you will observe how completely, so far as they go, the appearances correspond with those which attend the healing of an issue made with the same caustic; and how very slight is the disposition to the effusion of any plastic materials.

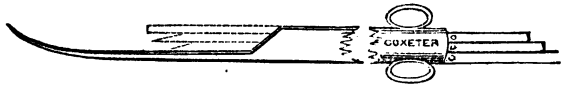
The first case on which I tried the caustic, now some years ago, gave me a very favourable idea of the benefit to be derived from this plan of treatment.

A strong-built middle-aged man, who had been a sailor, and afterwards a cobbler, had injured his perineum while serving in the navy. I presume, from his account, that extravasation of blood to some extent had taken place. Stricture began to form, for which he was treated by bougies, but with only temporary relief. The last time he had been under a surgeon's hands, no instrument could be introduced into the bladder. He progressively got worse, notwithstanding any temporary benefit he may have derived from treatment. When he came to the hospital, he was suffering from constant desire to pass his urine, which only dribbled from him. The urethra could be felt in the perineum hard and corded for at least an inch in length; the attempt to pass a catheter brought on severe spasm, and was followed often by rigors. The necessity for speedily relieving this state of things was evident; and I determined to use the caustic potash, rather, I believe, with a view of removing the extreme irritability, than of destroying the stricture. The caustic was applied in the manner recommended by Mr. Wade, and as you saw in the present case. The bougie was kept pressed against the stricture for about thirty seconds; and the operation gave rise to very little pain—less, certainly than the unarmed bougie had caused. As in the present case, it was found that, in the course of a day or two, he was far more comfortable; he had less difficulty in passing his urine, and less urgency. No unpleasant symptom followed the use of the caustic. I determined, therefore, to repeat the application; and this was done at the end of a week with increased benefit. After two more applications, the urine came away in a good stream; and a large bougie was passed without any difficulty into the bladder. I believe that strictures produced by mechanical violence are amongst the most obstinate of those with which we have to deal; but I have seen this man twice since; the last time was four years after the treatment by the caustic. On both occasions he had some spasmodic irritation, after too free living; but a full-sized bougie could be passed easily and at once into the bladder. I have no doubt that he would have returned, had he again suffered from his complaint.

It is only fair to say that the treatment of stricture by caustic has been condemned by many good surgeons, both British and continental; indeed, in France its use is almost unknown. Nélaton, for example, states that cauterisation is now rarely employed, in consequence of the uncertainty of its action; of the swelling of the urethra and retention of urine, which often result from it; and of the deposit of the retractile tissue which is formed afterwards, and which ultimately produces a new impediment. Here I have no doubt he is associating the effects of nitrate of silver with those of caustic potash—effects which, as I have stated, are very opposite. I have certainly seen no such effects, nor any tendency to them; and this is the conclusion at which Mr. Wade, with more extended experience, has arrived. I cannot help thinking, with that gentleman, that the treatment has been condemned rather from want of experience of its effects than from experience of its dangers. For a more complete exposition of these effects, and of the mode of applying the treatment, I must refer you to his works on the subject.—*Brit. Med. Journal, March* 2, 1861, *p.* 217.

62.—AN INSTRUMENT FOR CUTTING, DILATING, AND CAUTERIZING STRICTURE OF THE URETHRA.

It consist of a flat, metallic tube, seven inches long and a third or more of an inch broad, to one end of which there is



attached a grooved director of a size that will fill the channel of the stricture. Two or three long steel rods fitting this tube constitute the rest of the apparatus.

When it is used for incision, one of these rods ground for about an inch at the end into a cutting blade, is passed through the tube into the groove of the director, from which it is then pushed transversely and made to press upon and cut the stricture by one or both of the other rods. In cases of very narrow stricture, instead of a director, a cutting blade as small as is consistent with safety may be used, which is covered during the passage of the instrument by the blade of the steel rod in the manner of a pair of scissors.

Hemorrhage, urinary infiltration and pyæmia are consequences of the internal incision of stricture as hitherto performed by instruments which cut as they move forwards or backwards in the canal, making wounds of uncertain depth and extent. The new instrument cuts by pressure alone; or, when it is necessary to add a little sawing motion, this will not endanger the sound and elastic urethra, especially when

the knife is not very keen, as it is only as respects the resisting stricture that this motion is conjoined with the degree of pressure required to produce incision.

Solid dilators of transverse or eccentric action have been used by M. Perreve and others, but their numerous defects have prevented their general adoption, notwithstanding the superiority of their principle to that of instruments acting as wedges. The objection to the dilator of fluid pressure is the difficulty of making and using it, although its great advantages should compensate for this difficulty. When the new instrument is used for dilatation, a blunt rod is substituted for the sharpened one; and as the dilatation advances, an inch of it near the point may form half a cylinder of considerable size, lying upon, instead of entering the groove. The advantages of direct or transverse dilatation, whether by a solid or fluid body, were first pointed out, and are fully explained, in Dr. Arnott's works on stricture and stone.

When the object is cauterization, a grooved wire covered with nitrate of silver or potassa, is substituted for the cutting or dilating rod.

Although the great majority of strictures can be kept at bay by the common modes of dilatation, there are many hard, irritable, and elastic strictures which will not yield to these; and unless more efficient practices are resorted to, the patient is exposed to irreparable injury from the effect of continued irritation on the neighbouring organs. Unfortunately the more efficient practices hitherto employed are not unaccompanied with danger. Escharotics have fallen into disuse from the difficulty of confining their action; internal incision has the dangers already enumerated; and the perineal section, when restricted to cases in which other treatment is unavailable, has proved a very hazardous mode of procuring relief. The new instrument, by furnishing us with a better, and a very easily-manageable, means of dilating than the wedge, and a perfectly safe means of incision in cases not yielding to dilatation, supplies a great desideratum in this department of surgery.—*Med. Times and Gazette, Feb.* 2, 1861, *p.* 133.

63.—UNSUSPECTED ABSCESS OF THE PROSTATE GLAND, IN A CASE OF GONORRHŒA. WITH FEBRILE SYMPTOMS, FATAL RESULT.

Under the care of Dr. PITMAN, Physician to St. George's Hospital.

[Inflammation of the prostate gland, the result of acute gonorrhœa, sometimes runs rapidly into abscess—this result is usually announced by the occurrence of rigors—sometimes, however, nothing of the kind occurs, and the symptoms are very obscure. In the present case the abscess was not discovered at all during life, the patient sinking from the severe constitutional symptoms to which it gave rise. From the tumefaction caused by the tumour, instrumental assistance was necessary to evacuate the contents of the bladder.]

On referring to such standard works as those of Thompson, Adams, or Hodgson, on the "Enlarged Prostate," or Coulson on "Diseases of the Bladder," we can find no analogous case. The patient did not complain of anything that could have drawn attention to the seat of his disease, although an examination was made as far as seemed to be necessary under the circumstances. At the autopsy the abscess was found to communicate with the urethra, but evidently had not done so till very shortly before death; and the structure of the gland throughout the greater part of its extent was disorganized.

John E——, aged twenty-five, a baker, admitted April 25th, under the care of Dr. Pitman. He had had gonorrhœa for a fortnight, with pain across the loins. Four days before admission this increased in severity and extended down the legs, so that he had to discontinue work. He never had any shivering. On admission, he had much the aspect of a fever patient; the tongue was very dry, though of natural colour, excepting a white line all round the edge; the pulse was soft, 72. He did not complain of any pain, excepting a little in the legs, which was supposed to be rheumatic. There was a slight dry cough. He was unable to pass urine, except with the aid of a catheter. He was ordered to take the ammoniated saline with fifteen grains of chlorate of potass every six hours, and strong beef-tea was given.

May 1st. The surgeon was now requested to examine him; wine was given (six ounces); and as nothing had been elicited by the examination, the former treatment was continued. He was delirious at night, and in a state of great prostration, with involuntary evacuations. The skin was very hot, and still free from eruption or change of hue; the tongue dry and cracked; the pulse jerking and full, but utterly powerless, and with an aneurismal thrill under the finger.

3rd. His only complaint was of thirst. He gradually sank: ceased to swallow in the evening, and died towards morning.

Post-mortem examination.— The body was in good condition. There was profuse purulent discharge from the urethra, which had collected about the glans penis. The lungs were healthy. The heart was large; there was a large fibrinous vegetation on one of the flaps of the aortic valve, and the aorta was slightly atheromatous. Liver and spleen healthy, the latter rather soft. Right kidney healthy; in the left, the pelvis and ureter were rather more vascular than usual. Peritoneum healthy. The mucous membrane of the urethra was examined from its commencement to its termination: it seemed slightly injected in parts, but this appearance was so doubtful that little reliance is to be placed upon it. The muscular fibres of the bladder seemed rather thicker than natural, but its mucous membrane showed no obvious trace of inflammation. Between the bladder and rectum was a large abscess, which at the time of the examination communicated with the floor of the prostatic urethra by two rugged orifices. The tissue of the prostate gland was extensively destroyed

and eroded by the abscess, and on squeezing the abscess, pus could be made to exude by the prostatic ducts; but in parts near the abscess the tissue of the prostate seemed healthy. The cellular tissue in the neighbourhood of the abscess was condensed; in other parts of the pelvis it was healthy.—*Lancet, Oct.* 27, 1860, *p.* 408.

DISEASES OF THE SKIN.

64.—ON PARASITIC AFFECTIONS OF THE SKIN.

By Dr. T. Mc'Call Anderson, Fellow of the Faculty of Physicians and Surgeons of Glasgow.

[It is now thought by many that Herpes circinatus, Sycosis, and Tinea tonsurans are only varieties of the same disease, resulting from the difference in locality. In the majority of cases the destruction of the parasite is the *sine quâ non* of the treatment. It is necessary to remove the hairs, as the parasite penetrates into their interior, and is found embedded amongst their longitudinal fibres. This renders them very brittle and liable to break. The most successful mode of extracting the hairs is by means of a pair of broad-pointed forceps.]

If the diseased surface is not very extensive all the hairs can be removed at one sitting. As a general rule as much as three or four square inches of surface covered by hair can be removed daily; so that even were it necessary to remove the whole of the hair from the scalp this may be done in a week or so. But the extent of surface capable of being cleared at one sitting will depend entirely on the patient, those who have very sensitive skins being very intolerent of depilation, in which cases we must be content with slower progress, the depilation extending over a period of three or four weeks according to the degree of intolerance and the amount of hair to be removed. In France, where this treatment first assumed a definite form, great stress is laid on the manner in which the depilation was performed; and, indeed, in St. Louis Hospital, servants are employed (*épileurs*, as they are called) whose whole duty consists in extracting the hair of those patients affected with vegetable parasitic diseases.

Their *modus operandi* is thus decribed by Bazin:—"Our depilators are seated, and cause the head of the patient to rest upon their knees. With one hand (generally the right) they hold the forceps as one holds a writing pen—the other hand is applied to the part about to be depilated, with the thumb and index finger of which they put the skin on the stretch to keep it steady. They then extract the hairs, pulling them out in the direction of their axis, and only a small number at a time, two, four, six, or at most a small bundle. It is necessary to avoid depilating too quickly or too gently, there being an intermediate point which one can only arrive at after a little practice."

Before commencing the extraction of the hairs it is of course necessary to cut the hair short, say within an inch of the skin, and by means of poultices to remove any crusts which may have formed, and are matting the hairs together. Bazin also recommends a layer of the oil of cade, which he says "destroys, in part, the parasite situated on the surface of the skin, extinguishes the sensibility of the scalp, and facilitates the extraction of the hairs." Did the oil of cade fulfil these conditions, it would certainly be of great value in the treatment by depilation. That it destroys some of the spores no one can doubt; but from what I have seen of this practice, I would be very chary indeed of admitting that it in any way deadens the cutaneous sensibility or facilitates the extraction of the hairs.

A question of great importance is, *When should the depilation be stopped?* There is no doubt that one depilation would be sufficient were all the hairs removed entire, and the parasiticide efficiently applied; but there is great difficulty in removing all the hairs, owing to their friability, and the depilation thus requires frequently to be repeated. Each successive depilation, however, becomes less serious; for we generally notice, after one extraction, that the disease is extinguished at some parts, and reappears only here and there, at those places namely where the hairs have broken in attempting to remove them. It is only these latter parts, then, which call for a repetition of the operation. After a little experience one can readily distinguish when the disease is eradicated, and when the treatment can be dispensed with. When the hairs shoot out with all the appearance of health, after depilation, their previous friability and twisted appearance being gone, and when the skin assumes its natural appearance, we can then withhold the forceps, and confidently expect a permanent cure. It must, however, be borne in mind that, long after the spores have been destroyed secondary symptomatic eruptions may continue which do not require further depilation, but treatment founded on general principles, and varying according to the nature of the eruption. It requires very little clinical experience to distinguish the symptomatic eruptions from those peculiar to the parasitic affection.

Having now described the method of extracting the hairs, it remains to mention the best parasiticide applications. Of these the bichloride of mercury is one of the best, and the one most generally used; oil of cade and Turbith mineral are much employed in France. Sulphur is also good, although more successful in the destruction of the animal parasites. As for the excipients, water and glycerine are probably the best. Lard is also good, provided that care be taken to get it fresh, and not to keep it too long; for, if rancid lard is applied to the affected parts it favours, in a wonderful manner, the formation of secondary eruptions. The strength of the parasiticide to be used is a point of great importance. Two grains of the bichloride of mercury to an ounce of water is sufficiently strong, and a little alcohol or muriate of ammonia should be used to facilitate the solution of the mineral. Sulphur

is best used in the form of ointment, in the proportion of half-a-drachm to a drachm of sulphur to the ounce of lard. Turbith mineral is usually employed in the same proportion, and lard is the general excipient. Oil of cade may be mixed with glycerine or lard in the proportion of half-a-drachm to a drachm in an ounce of the excipient.

Whichever of these is employed, must be rubbed into the skin *during* each depilation, as previously mentioned, and *after* the depilation is completed, and the disease apparently cured, it should be continued for some time. The disease should not be pronounced cured till a considerable time has elapsed after the cessation of the parasiticide, and that for obvious reasons.

After the cure of the parasitic affection, symptomatic, especially impetiginous eruptions may be left, which must be treated according to their nature, and as they are produced by the irritation of the parasite or of the remedial applications, soothing remedies are generally the most useful. But, sometimes, the irritation which the disease produces is the means of calling out some latent diathesis (*e.g.*, an eczema, a psoriasis, or a syphilitic eruption), in which case, general remedies must be employed, in order to correct, if possible, that particular state of system.

Finally, we must, if possible, remove the predisposing causes of the disease. The nature of these causes has, as I have said, not yet been thoroughly investigated, and we must, therefore, at the same time as we attack the disease locally, attend to the general health, and be guided in this by broad, general principles.

It now remains for me only to say a few words with regard to the particular treatment of each of the varieties of Tinea tonsurans, and also of the other modes of treatment very generally in use in this country.

In cases of *herpes circinatus* (ringworm of the body) the application of a parasiticide is generally sufficient, as the down which usually covers the skin of the body is not impregnated with the spores, except to a very partial extent. In very hairy persons, however, it may be necessary to proceed to depilation, especially if the hairs are manifestly diseased. Herpes circinatus is an affection which not unfrequently gets well of itself, the parasite not having, apparently, the same hold as on the hairy parts of the body. When there are many patches of herpes circinatus on different parts of the body, it is best treated by sulphur or mercurial vapour baths, which act on the whole skin, and thus attack all the patches at once.

Sycosis must be treated both by depilation and parasiticides. When the diseased process has run very high and there is a great deal of suppuration and induration, it is, according to some, better to commence the treatment by soothing applications such as cataplasms of potato-starch combined with purgatives. I am not quite certain, however, whether it is not better to proceed to depilation at once, for the best way of removing the irritation is to remove the cause, and, besides, the hairs are much more easily extracted while the inflam-

mation and suppuration are considerable. In the early stages of the disease, depilation causes considerable pain, and, although well performed, some of the hairs usually break, leaving their diseased bulbs in their follicles, so that a second or third depilation, although only a partial one, is generally required. When the disease is treated in the advanced stages, after the formation of pustules and indurations, although the inflammatory and suppurative processes have destroyed the parasite almost completely, the depilation is just as necessary. For, although the fungus is all but destroyed, the diseased ends of the hairs remaining in their diseased and abnormally secreting follicles, act as foreign bodies and keep up the inflammation. At this advanced stage of sycosis, the results of depilation are certainly *marvellous*. The hairs are removed with the utmost facility, and one depilation is generally sufficient; but it must be combined with the application of a parasiticide lotion or ointment. It is almost incredible, to those who have not witnessed cures from this treatment, the rapidity with which a chin, twice its ordinary size, and covered with large tubercles, indurations, and crusts, becomes, in the space of two or three weeks, perfectly healthy in appearance, with this exception, that here and there patches of alopecia are left, which are permanent; but this is not the fault of the treatment, but the fault of the patient in neglecting to apply for advice in the earlier stages of the disease. In the treatment of sycosis, many are in the habit of applying strong caustics, a mode of treatment which cannot be too much reprobated; for unless it be carried to such an extent as to destroy the skin and hair-follicles, and thus produce great disfigurement, it is not and cannot be successful. I am glad to think that this mode of treatment is becoming more rare in the present day. Arsenic has always been a very favourite medicine in the treatment of sycosis, and its use, combined with cod-liver oil, is highly extolled by Hunt, who seems to look upon it almost in the light of a specific. I can easily conceive that this remedy, so powerful in many other cutaneous affections, may act beneficially in changing the state of the system favourable to parasitic development, although I have never had occasion to try it; but I can hardly think that it can cure the disease, unless, by changing the composition or qualities of the blood, it causes the death of the parasite. Besides, in examining the illustrative cases cited by Hunt, I am left in doubt as to the nature of the affection. Thus, some are described as labouring under a tubercular eruption of the bearded portions of the face, others as having been troubled with sycosis one year, and in another case the only thing we are told with regard to it is that *the* disease had existed "on and off," for fourteen years. And on looking at the results of the treatment I find that one patient was "nearly well when he last applied," another is "believed to have recovered," and a third "absented himself, *probably* cured"!! Now, however much Mr. Hunt may have convinced himself of the efficacy of arsenic and

cod-liver oil in the removal of sycosis, it is necessary to have more convincing proof than he has given us of its beneficial operation. And, even supposing that Mr. Hunt's treatment is capable of removing sycosis, it is not nearly so successful even by his own showing as the treatment by depilation and parasiticides.

In *Herpes tonsurans* (ringworm of the head) depilation and parasiticides form the best treatment. It is, however, the most intractable of the three varieties of tinea tonsurans, owing to the friability of the hairs, and the difficulty of extracting them entire. Depilation requires, therefore, to be frequently repeated and the cure is thus much retarded. This can, however, be always anticipated with certainty. Caustics have been much used in this affection, as in sycosis, but the remarks made with regard to them when treating of the latter apply equally to Herpes tonsurans. Shaving off the hair from the diseased parts, followed by the application of stimulants and combined with tonics internally, are often made use of, but the results of this treatment are not very satisfactory. Dr. Jenner recommends strongly an ointment composed of twenty grains of the ammonio-chloride of mercury, and four drachms of sulphur ointment, which is certainly a powerful parasiticide. He states, however, that the removal of the hairs is *impracticable*, a statement which is far too sweeping, although there can be no doubt that the hairs break very frequently, and that great patience and repeated efforts are required before a satisfactory depilation is attained.

[Speaking of the *Diagnosis*, the author observes:]

This is, in the majority of instances, very simple. In cases where the head is more or less covered with an eruption exhaling the odour of mice, and consisting of bright yellow dry crusts depressed in the centre, through the middle of each of which one or more hairs pass, which have a dull dry appearance, and are more easily extracted than natural, the diagnosis is perfectly simple; and those who have seen the disease once can never mistake it. When it has continued for a length of time, when the crusts have lost their cup-shaped form and their bright yellow colour, and have become entangled in the hairs; when, in fact, we have to do with the variety described as favus squarrosa, it may be—and often is—mistaken for impetigo of the scalp. But in the former there are generally *patches* of alopecia, which are wanting in the latter. In it certainly the hairs often fall out, although only *here and there* and not in *patches* as in favus. The alopecia of favus is *permanent*, that of impetigo generally *temporary*. There is also no alteration of the hairs in the latter; in the former they are dull, dry, discoloured, and easily extracted. Attention to these points generally serves to clear up the diagnosis; but if doubt still exists, it may at once be removed by the microscopic examination of the crusts. There is one point, however, which requires to be borne in mind, namely, that the discovery of some pustules does not prove that the disease is

impetigo, as pustules are frequently developed in cases of favus from the irritation of the parasite. And also one should not lay too great stress on the value, in a diagnostic point of view, of the odour exhaled from the eruption; as this symptom is not so pathognomonic as some dermatologists would lead us to suppose.

Psoriasis of the head might in some cases be mistaken for favos when this latter is not well marked. But in the former there is no alteration of the hairs, no alopecia, and no peculiar odour. The crusts, also, are much whiter, are more adherent than in favus, and the characteristic silvery scales of psoriasis are generally to be detected on other parts of the body, and especially on the elbows and knees, for psoriasis confined to the head is rare. In doubtful cases the microscope at once determines the nature of the affection.

Very often the diagnosis is rendered difficult on account of a propensity of parents to clean carefully, and remove all the crusts from the head before bringing their children for advice. There is then to be seen redness of the scalp combined with the presence of a few pustules, the results of irritation, and which makes the disease resemble impetigo. But if it is a case of favus which we have before us the *deep red, depressed*, distinctly *circumscribed* surface, covered by a *thin, shining* epidermis, is quite different from the light-coloured, diffused redness of impetigo. If this is not sufficient, the hairs should be examined, when they will be found to be altered, and the parasite is detected in them with the microscope. If this is not satisfactory, do not give an opinion, or resort to any treatment, but desire the patient to return in a couple of weeks, leaving the head untouched in the *interim*, after which time the disease will have had time to re-develope itself, and its nature is at once discovered. Favus, attacking the hair-follicles of the body, is easily detected, as the crusts invariably assume the cup-shaped form, and the epidermic variety of the disease, and favus of the nails, if not over-looked altogether, are easily diagnosed, provided always that we keep in mind the characters pertaining to each, as given above, and do not forget the fact that they occur, almost invariably, secondarily to the favus of the hair-follicles.

The *Prognosis* which in former times was very serious, owing to the supposed incurable nature of the disease, is now, on the contrary, very favourable; for it has been satisfactorily proved that, by the aid of the treatment about to be recommended, a cure, even of the very worst cases, can invariably be effected. If treated in its early stages, recoveries take place without leaving the slightest deformity, but when the treatment is commenced after the disease has lasted a length of time, the alopecia and the atrophy of the scalp, which it produces, can never be removed. Cases of death from pure favus have always been rare, and should never occur in the present day, and, indeed, it is only after it has lasted a very long time, that it affects the general health to any extent.

Treatment.—It can easily be imagined that a disease such as favus,

which can be traced back to the remotest ages, and which, till of late, was deemed incurable, should have called forth very varied and opposite principles of treatment. Some considered it to be essentially a constitutional, in fact a scrofulous affection, to be removed by constitutional treatment only; others were of opinion that it was purely a local disease to be removed by topical applications alone; while a third party looked upon it as a disease partly local and partly constitutional, and attacked it both by local and general remedies. The local treatment has, however, at all times and in all countries, played a more important *role* than the constitutional. When no treatment is adopted it is very rare indeed that the disease disappears, unless after it has destroyed every one of the hair-follicles of the scalp and produced a permanent alopecia. But even then, it may continue to flourish on other parts of the body. When left to itself it generally lasts more or less extensively during the whole life of the person affected.

The effect of an acute intercurrent disease, a typhoid fever for example, on the favus affection is very curious. During its continuance the achorion does not flourish, but fades as plants do, when the soil in which they are planted is not supplied with moisture, or is otherwise inappropriate, and it only assumes its pristine vigour after the disappearance of the fever. Of the many varieties of local treatment adopted, that by means of the *Calotte* is the most celebrated, and is still practised in France to a considerable extent. This consists of a plaster which is prepared in the following way: "About four ounces of rye-flour are mixed with a pint and a-half of white vinegar in a pan, this is then put on the fire, and the mixture stirred continually; to it, when it boils, is added half an ounce of the carbonate of copper in powder. The whole is then boiled for an hour, after which about four ounces of black pitch, six of Burgundy pitch, and four of resin are added; when thoroughly mixed and melted, about six ounces of antimonial ethiops are put in, and the mixture is stirred continually. till it has assumed a suitable consistence. This plaster is then spread upon strong cloth. It is employed thus:—The hair is cut as short as possible, the crusts removed by means of cataplasms, and the plaster applied in strips, so as to fit the head accurately. It is left on for a few days, so that the hairs in growing become firmly adherent to the plaster. The end of each strip is then seized and suddenly and forcibly removed, dragging along with it a number of the hairs. This operation is repeatedly performed, and extends over a period of several months in many cases. The practice has sometimes proved successful; but only so when, by it, the whole of the diseased hairs are removed, but unfortunately, in many cases, this cannot be accomplished, and in too many others the operation is too painful and cannot be borne, and instances of death from this cause have been recorded. It is adopted, however, by Baumes, of Lyons, and by the Sisters of Charity at Toulouse, in which town, curious to relate, the municipal administration has interdicted the physicians from treating favus, the care of the children so

affected being confided exclusively to the Sisters of Charity. This absurdity is carried to a great extent in Paris also, where the St. Louis Hospital counts the Freres Mahon (the quacks) among the medical staff. This was somewhat excusable many years ago, for then the parasitic affections of the head were certainly better treated by them than by others; but now that the treatment proposed by Bazin and adopted by Hardy is proved to be better than that of the Freres Mahon it seems to be highly improper, and a great insult to the distinguished physicians of that hospital, that such charlatans should still be retained in office. For of all kinds of quacks, those are decidedly the worst, who, having discovered a useful remedy for a loathsome disease, prevent the public from profiting by it, retaining it as a secret, for the purpose of advancing their own interests, and filling their own pockets. The process of the Freres Mahon is somewhat the following:—The hair being cut short and the crusts removed by poultices and soap and water, *a marvellous powder* (to use Bazin's expression) is applied and the hairs are then removed by combing, and by pulling out as many as possible with the fingers. Bazin remarks that the epilation by this means is often impossible, in which case they (the Freres Mahon) have no scruple in having recourse to his method of epilation with the forceps. According to the analysis of Chevallier, the depilatory powders of Mahon consist principally of slacked lime and the subcarbonate of potash.

The *best* way of curing favus is *decidedly* that by depilation and the application of parasiticides, as proposed and adopted by Bazin, and followed by Hardy and others. I have already explained so fully this method of treatment, when on the subject of tinea tonsurans, that it is not necessary to enter upon it here, as what we have said with regard to the treatment of the latter applies in every respect to the disease under consideration. The length of time required for a cure will vary of course according to the extent of the diseased surface. When it is very limited, a cure is effected in a few minutes, as in the following cases, cited by Deffis in a pamphlet bearing this eminently French title—"Refutation of the Errors which M. Devergie's Book contains:"—

"March 16, 1854.—Leon Dufour, aged six months. Two cup-shaped favus-crusts on the back part of the head. Cured in a single sitting, on the 16th March, 1854.

"December 9, 1854.—Julie Laporte, aged 10 years. One favus-cup on the summit of the head. Cured in a single sitting, on the 9th December, 1854.

"March 30, 1855.—Alfred Hubert, aged 6 years. Four small favus-cups on the scalp. Cured in a single sitting, on the 31st March, 1855."

In these cases the number of crusts is certainly very small, but notwithstanding, there is no treatment but that under consideration which is capable of producing a *radical* cure in nearly so short a space

of time. When the disease is very extensive, affecting the whole scalp, the depilation of the whole diseased surface must of course be undertaken and often repeated; and in such cases the duration of the treatment may extend over a period of from six to twelve weeks, in which the most obstinate cases should be *radically* cured; and this is what can never be effected under any other treatment whatsoever. I am well aware that in hospital practice many physicians are content with removing the crusts, and applying stimulating washes, attending to the general health, and dismissing the patients with *clean heads*, and *apparently* cured, after a couple of weeks' treatment; but these are not instances of cure at all, for whenever the local applications are stopped the crusts reappear. No favus can be said to be radically cured unless the head remains perfectly healthy for some months after the cessation of all local treatment. Those, however, who are conversant with the disease, and the treatment by epilation and parasiticides, soon learn when to pronounce a cure; for to an experienced eye the appearance of the skin of the scalp and of the hairs, more especially, is a sufficient index of the eradication or non-eradication of the disease. But on these points it is not necessary to enlarge further, for they have all been discussed previously. The method of removing the favus crusts before proceeding to epilation is by the use of cataplasms, or by the application of oil, which softens them, and causes them to *swell* and lose their attachments to the skin, after which they may be scraped off with a spatula. This is the method adopted by Hebra. Instead of oil, he sometimes makes use of alcohol, which has exactly a contrary effect on the favus crusts, causing them to *shrink*, and thus lose their attachments, when they fall out, or are removed with the spatula. With regard to the parasiticide lotions and ointments the remarks made with regard to them under the treatment of tinea tonsurans apply equally to favus. Küchenmeister made a series of very interesting experiments, in order, as he says, "to test the parasiticidal effects of the most urgently recommended remedies," and he found that *alcoholic solutions* acted most powerfully in this manner:—At his request applications of alcohol were made to the heads of some patients affected with favus by Hebra, and apparently with great success. Notwithstanding this statement of Küchenmeister's, I find in a later publication that Hebra considers the application of the so-called parasiticides as useless, there being, he says, no medicine which is known to act in this way. This opinion is not shared, however, by most dermatologists.

Bennett, who is of opinion that the disease is parasitical, but occurs in scrofulous constitutions, recommends the use of cod-liver oil internally. Locally also, after the removal of the crusts with poultices, he applies cod-liver oil, which, as he says, by excluding the air, causes the death of the parasite. I am told, however, by my friend, Dr. Stewart, of Edinburgh, (formerly Dr. Bennett's clinical assistant), that this treatment is merely palliative, and certainly the cases cited

in Dr. Bennett's valuable work on Clinical Medicine (Second Edition pp. 700-794) are not very encouraging.

After what has been said, it will be seen that the local is the only treatment capable of effectually curing favus; but at the same time, in this, as in all local diseases, the general health must be attended to, and any deviations from a natural state corrected when possible. Cleanliness must, above all, be insisted on, combined with the use of good food, and the enjoyment of exercise, pure air, &c. The principles of treatment above sketched out are always efficient in curing even the worst cases of favus; and we hope that, as it comes more extensively into practice, cases of favus described as *incurable* will cease to be recorded, for since the depilatory treatment of Bazin has been instituted, the word *incurable*, when applied to favus, has become quite inapplicable.

The remarks above made apply to favus attacking the hair-follicles. In the epidermic variety depilation is not necessary, the application of a parasiticide lotion, as a solution of the bichloride of mercury (two grains to the ounce of water), being quite sufficient to remove it. When the disease attacks the nails the treatment is also simple. It is necessary to destroy gradually by means of a small file the portion of nail covering the favus matter, and after arriving at it the application of a parasiticide is quite sufficient to destroy the fungous growth.—*Med. Times and Gaz., Feb.* 2, *and March* 9, 1861, *pp.* 112, 247.

65.—OBSERVATIONS ON FAVUS.

By Dr. WILLIAM PIRRIE, Jun., late Assistant-Surgeon H.M.'s 71st Highland Light Infantry.

Favus is an affection of the skin usually confined to the scalp, but occasionally found on the trunk, limbs, or face; almost invariably occurring before the seventeenth year, and peculiar to the poorer classes. It is characterised by the presence of more or fewer crusts, which, at first, are of a uniform sulphur-yellow colour, have their superior surfaces slightly concave and pierced in the centre by a hair, and their edges slightly depressed beneath the level of the cuticle, and are surrounded by skin which, for some time previous to their appearing, presents a glazed, red, and vascular look, and is more or less covered with desquamated epidermic scales. The crusts, when isolated, have a more or less distinctly round or oval figure, and have in their upper surfaces a depression, in form like that of a lupine seed (porrigo lupinosa); but when numerous and confluent, they acquire, from contact with one another, a hexagonal or honeycomb appearance, and hence the term porrigo favosa. In the progress of development, the edges of the crusts become prominent, and marked by several concentric rings, whilst their upper surfaces gradually lose the concave and assume the convex form. In course of time, there may be obser-

ved in the centre of each capsule a whitish spot, which slowly extends into the surrounding deeper yellow portion, and ultimately reaches the edges of the crusts, which gradually sink to the level of the skin, and frequently present numerous cracks or splits. There speedily commences in this central white portion a loss of coherency, or a process of crumbling down into a coarse gritty powder; and when this, in the progress of the disease, has extended to the edges, the capsules lose all definite form, and become broken or split up into numerous pieces, presenting the appearance of one continuous crust irregularly fissured on the surface. The diseased mass crumbles down, and communicates to the fingers, on trituration, a feeling very aptly compared by some to that caused by the crushing of dried putty; becomes a favourite site for vermin and their eggs, and exhales a peculiar odour, which has been likened to that of cats or mice. For some time the disease occasions little uneasiness; but ere long the part affected becomes the seat of a disagreeable itching, which, gradually increasing, at length becomes so intolerable that the patient cannot resist incessantly scratching and tearing at the diseased mass, the consequence of all which is, that in cases of some standing, we usually meet with more or less sanguineous or serous effusion, with secondary pustular eruptions, and at times with violent inflammation, which in a few cases terminates in suppuration, and in other instances in unhealthy ulceration.

Such was the form and such the sequel of phenomena exhibited by all the examples of the disorder which came under the writer's observation; but we have it on the authority of Dr. Bennett that, on some few occasions, the disease, instead of assuming the form already described, becomes disseminated underneath the epidermis, presenting all the outward appearances of pityriasis or chronic eczema, from which affections it is to be diagnosed only by microscopic examination, which brings into view thalli and sporules in various stages of development. With reference to this variety of the disease, the authority already named remarks:—"In one instance I found the sporules smaller than usual, and perfectly globular, instead of oval. In others, I have seen the sporules three or four times larger than those of ordinary favus with included nuclei, multiplying fissiparously. Hence the so-called parasitic pityriasis of the scalp I believe to be a modification of favus, and consider it a good rule, in all chronic eruptions on the head, to examine the crusts microscopically."

I would here remark that the admission of parasitic pityriasis as a modification of ordinary favus must greatly affect the generally expressed opinion regarding the rarity of the affection.

Pathology.—When a recently formed favus crust is carefully examined, it is found to consist of a capsule of epidermic scales, coated in the interior with a finely granular matter, constituting a soil on which arise multitudes of a peculiar fungus, named achorion Schonleinii by Gruby, in 1841, in honour of its discoverer, Schonlein, of Berlin. From the granular mass sprout numerous cylindrical

tubes (thalli), which extend themselves towards the centre of the crust, and, branching dichotomously, contain at their extremities (mycelia) numerous spherical or oval bodies termed sporidia. The thalli frequently contain numerous molecules or granules, and are most numerous near the exterior of the crust; whilst the mycelia and sporules mingled with more or less granular matter, abound at the centre, the whitish colour of which has been ascribed to the aggregation of the sporules. In course of time the thalli and sporules enter the hair follicles, cause atrophy of the hair-bulbs, and consequently the baldness observed in the subjects of favus, and, as I have myself witnessed under the microscope, at times extend into the body of the hair. The sporules are the bodies from which the plant is developed, and, from the observations of Remak and others. it appears that, in the process of elimination, their investing membrane gives off shoots or prolongations, which ultimately become tubes, enclosing at their extremities sporules, which in time are pressed out and become free. Seeing that the peculiar favus matter is found in relation with the derma and the epidermis, filling up the hair-follicle, and at times extending into the body of the hair, it becomes an interesting question to determine what tissue is the real seat of the disease.

On this point the following four opinions have been entertained :—

1st. Some, as Baudelocque, Duncan, Hutchinson, and Willis, consider it as a disease of the hair follicles or hair-bulbs.

2nd. Others, as Bateman, Thomson, and Rayer, give it as their opinion that the disease is at first confined to the reticular tissue of the skin.

3rd. Others, as Murray, Mahon, and Cazenave, state that the disease is located in the sebaceous glands.

4th Gruby thinks that the plants grow in the epidermic cells, that the true skin is not destroyed, and that the hair-bulbs and sebaceous follicles are only secondarily affected; and Bennett conjectures that the sporules from which the fungi are developed penetrate into the crevices and underneath the portions of half-detached epidermis, and that they do not spring up at first either below or in the thickness of the cuticle.

From the opportunities afforded me of studying the disease, I have formed the impression that the peculiar matter of favus is an exudation on the surface of the derma; that this exudation becomes the seat of peculiar phytaceous growths, which, in the progress of development, penetrate the epidermis and become encysted by its scales; and that the hair-follicles are not the seat of the disease, but only become secondarily affected. That the air-follicles not the seat of the disease I have concluded from observing—

1st. That on carefully removing recently-formed favus crusts from the scalp, the only thing to be seen was a slight depression on the derma without any abrasion, which would have been expected were the crust an exudation in the hair-follicle. In the centre of this

depression may frequently be observed the open mouth of a hair-follicle, which is more deeply placed in the reticular portion of the corium proper.

2nd. That it is possible to raise from the scalp a crust of short duration, and to draw it over the hair, leaving the latter standing in its follicle.

3rd. That permanent baldness does not invariably follow favus, which would reasonably have been looked for were the hair-follicles the primary seat of such a serious disease. In some instances, the hair, though weak and altered in texture and colour, does appear after the removal of the complaint on the parts which were affected. It has occurred to me that in many cases of favus which have not been left long without treatment, the hair-follicle, from being below the original seat of the exudation, is not obliterated or destroyed, but only impeded or impaired in function, and the hair in consequence temporarily destroyed; but that, on the removal of the disease, the follicle again resumes its wonted function. and the hair in time becomes regenerated.

4th. That favus on some few occasions occurs in parts where it is impossible to detect any hairs piercing the crusts. Thus in one case under my father's care in the Royal Infirmary, there were several large well-developed crusts over the right shoulder-blade of a little sickly boy about nine years old; and in this instance, not only were the capsules not formed around a central hair, but no hairs could be observed penetrating them at any point.

[That favus can be communicated by inoculation, is placed beyond all doubt by the experiments of Bennett, Remak, and Mahon, and that it does at times spread by contagion is attested by many practitioners. But the question, whether the disease does not often arise without any contagion, is still unanswered. The author has seen five cases, none of which could be traced to contagion, and he believes, that favus very often arises without any contagion, and if contagious, is very feebly so. It is probable that favus has a very intimate connexion with the strumous habit, for the following reasons:]

1. In all the cases there were marked impairment of health; decided depression of the general powers; a thin, weak, and badly-nourished state of body: a sickly white colour; much gastric derangement, more especially acidity, and great irregularity of the bowels. In fact, the general appearance and condition of the sufferers were such as warranted the observers in at once pronouncing them of the scrofulous diathesis.

2. Although the general aspect of all was decidedly strumous, in four there were the following more glaring manifestations of that constitutional taint:—Enlargement of the cervical lymphatic glands, of much older date than the eruption; scrofulous enlargement of the sub-maxillary gland; cicatrices of old scrofulous sores on the arm, and incipient pulmonary tuberculosis.

3. On inquiring more particularly into the histories of the cases, more or less of a scrofulous connexion was always discovered, some one of the patient's family or relatives either being at the time of investigation, or having been affected with some form or other of scrofulous complaint.

4. All the parties affected had been living, for periods of longer or shorter duration, in circumstances well known powerfully to predispose to the strumous diathesis, such as exposure to cold and damp, hardships, overcrowded dwellings, low and confined localities, imperfect or neglected ventilation, and improper and deficient food. One circumstance particularly worthy of notice in regard to the food of the lower classes in this part of the country is, that oatmeal forms the staple article of diet, and that in the case of very many, it constitutes, in the form of porridge or of cake, the three meals of the day. In the case of others, the afternoon repast is varied by barley broth, either plain, or containing some green vegetables, and, at times, scanty fragments of lean meat; but one feature common to all their diets is the exclusion of the oils, the very substances which form our sheet-anchor in the treatment of scrofulous affections.

5. None of the cases were cured by local applications independently of constitutional treatment.

But as many hundreds of persons placed in the same circumstances suffer from phthisis, or some other form of tubercular disease, and yet never become affected with favus, there must surely be some peculiar and powerful predisponent to the disease. I have thought that this might be arrest or retardation of the cutaneous functions; for one feature common to the histories of all the cases which have come under my notice is that the sufferers had been living, or more properly, in the case of some, had been allowed to live, in total neglect of cleanliness, almost perfect strangers to ablution, and in a state of perpetual filth. From all these circumstances I am disposed to think that favus is a peculiar and not very usual manifestation of the scrofulous diathesis; that the determination to the skin is owing to perverted nutrition of that tissue from arrest or retardation of its functions caused by long-continued want of cleanliness, or some other irritating agency; that, though traced in some instances to contact, still it often arises without contagion; and that the parasitic fungus is a spontaneous growth in the peculiar exudation which constitutes a soil congenial to its development.

Treatment.—Not to mention the various measures and appliances recommended for the removal of the hair by those who consider favus to be a disease of the hair-follicles, two distinct methods of treating the complaint may be briefly noticed.

1. Some, who consider that the disease is not associated with any particular diathesis, and whose experience it has been to meet with examples of it in states of moderately good or even rude health, look upon constitutional remedies as altogether unnecessary in ordin-

ary cases, and rely for the removal of the affection entirely on local applications, recommending the adoption of certain substances believed to possess the power of destroying vegetable life, such as mercurial ointment, sulphur ointment, sulphur and ammonio-chloride of mercury ointment, creosote, and various other preparations.

2. Others insist on the necessity of administering constitutional remedies conjointly with the employment of local means, and follow and recommend this method of treatment from the belief that the growth of the plants already formed may be destroyed by the adoption of certain local measures, but that the existence of the exudation or soil congenial to the development of the cryptogam is owing to a peculiar vitiated condition of system which must be corrected by appropriate means before a continuance of the disease can be prevented.

The two great indications to be fulfilled by local remedies are—

1st. To remove the crusts from the portion or portions of integument affected.

2nd. To prevent the development—to destroy the life of the fungus.

The first indication is very easily accomplished by the use of warm poultices, or other emollient applications.

The fulfilment of the second indication has been attempted on two different principles, both of which have undoubtedly been effectual in many instances:—

1st. Some have succeeded in checking the growth of the fungus by keeping constantly applied to it certain irritating substances, as creosote, sulphur ointment, sulphur and ammonio-chloride of mercury ointment, sulphurous acid lotions, and other preparations, the beneficial action of which is attributed to their having inherent in them certain properties powerfully and directly injurious to vegetable growth.

2nd. Others have been equally successful in effecting the death of the parasite on the principle of entirely and steadily excluding the air from it, and for this end they use substances which, in themselves, are by no means injurious to vegetable life. Thus Bennett instituted the local application of cod-liver oil with this view; and expresses the belief that common lard or any other oil would be equally efficacious.

As all the cases which have come under my observation here have been distinctly connected with a cachectic or scrofulous habit of body, one important indication of treatment has been the correcting of this condition by the employment of constitutional remedies. For this end, the adoption of all means calculated to maintain a proper condition of the digestive organs and to improve the general strength, the injunction of free ventilation and exercise in the open air, the rigid enforcement of personal cleanliness and the administration of nourishing diet, tonics, and cod-liver oil, are indispensable. In one word, in the various cases which I have carefully watched under my father's treat-

ment, and in one or two under my own particular care, I have found the energetic adoption of the anti-scrofulous regimen and remedies with all their details, together with the local application of cod-liver oil, recommended by Bennett, attended with such thoroughly satisfactory results, that I would unhesitatingly have recourse to their exclusive employment in any case for the future.

Summary of Conclusions.—1. That favus is essentially characterized by the presence of a fungus, which is easily discovered by the microscope.

2. That it is peculiar to the young, and confined to the poor and destitute.

3. That it is most commonly met with on the scalp, but occasionally on other parts of the body.

4. That the hair-follicles are only secondarily affected.

5. That it is by no means a rare disease in Scotland, being exceedingly common in Edinburgh, and having been more so for several years past in Aberdeen than in Glasgow.

6. That it is generally considered more common in Ireland than in England.

7. That it is a blood-disorder, and that the fungus is not the sole nor the original cause of the eruption.

8. That many are insusceptible to it; and that it is feebly contagious, and very often arises independently of contagion.

9. That the previous state of health has an important bearing on its outbreak.

10. That it is intimately connected with the strumous diathesis.

11. That want of cleanliness strongly predisposes to it.

12. That, for its removal, general as well as local treatment is necessary.—*Lancet, Dec.* 8 *and* 15, 1860, *pp.* 557, 582.

66.—*Tinea Favosa treated by the Carbonate of Copper.*—The salts of copper are becoming gradually introduced more largely into practice; and the above new application of them has been brought forward by M. Huet, physician to the Penitentiary for Juvenile Delinquents. A great number of children enter that institution affected with tinea favosa, and the good effects obtained in these cases, by the trial of his mode of treatment, has induced M. Huet to recommend it strongly. It consists as follows:—The head is carefully cleaned by means of unctuous cataplasms, and, when the crusts have been completely detached, it is shaved. The following ointment is then applied: Carbonate of copper, 2½ drachms; purified axunge, 15 ounces. After numerous trials, M. Huet considers this ointment as the means which cures porrigo with the greatest rapidity. Sometimes it is necessary to suspend the medicine, and return to the poultices for a certain time, before resuming the use of the carbonate of copper.—*Repertoire de Pharmacie—Bull. Gen. de Therap.*, 1861.—*Edin. Med. Journal, March*, 1861, *p.* 834.

67.—*Treatment of Prurigo.*—During the year 1860, sixty-eight cases of prurigo were treated in Professor Hebra's clinique. Most of the patients had been under treatment for the same affection on previous occasions. The disease, according to Hebra, occurs only in poor people, who pay little attention to the state of their skin. Any measures which will soften the skin will remove the papules and itchiness. The treatment recommended is warm baths with plenty of soap, and, after each bath, rubbing into the skin an ointment composed of equal parts of cod-liver oil and tar. The itchiness almost invariably ceases after the second or third application.—*Spitals-Zeitung, No.* I., *Jan.* 5, 1861.—*Brit. Med. Journal, Feb.* 23, 1861, *p.* 207.

68.—ON THE TREATMENT OF ECZEMA.

By Professor HEBRA.

[The following is extracted from a Clinical Lecture on the subject.]

One has the satisfaction of knowing that we can always cure eczema, however long it may have continued, although in some inveterate cases depending upon a dyscrasis, or upon internal causes, there may be considerable difficulties in the way. As in most other diseases of the skin, the treatment should be purely local, internal measures being limited to the rare cases in which the eczema has been produced by a previously diseased condition of the economy, or when it is combined with some other affection. In the great bulk of cases internal means, such as mercury, antimony, iodine, purgatives, sarsaparilla, &c., are superfluous and mischievous. Arsenic is the only one of such means which exerts any influence in obstinate cases. Ordinarily, however, it is of no use, and its employment should be limited to the few cases which manifest especial obstinacy, the local treatment even in these also not being at the same time neglected.

Cold water, in its various modes of application, is of great importance, combined with other means, in the treatment of eczema. Employed alone it is far too tedious, and frequently not sufficing in its operation. It only aggravates the malady when applied in *eczema simplex;* arising from excess of secretion, as *e.g.*, in the axilla, between the buttocks, &c. Starch, whether alone, or mixed with oxide of zinc (starch ℥j, zinc ℥iij) is, on the other hand, an excellent application when the eczema arises from the friction of two cutaneous surfaces, or from excessive secretion, as in the axilla, under the breasts, the scrotum, buttocks, &c. Oxide of zinc (℥j. to ℥j of lard), sulphate or acetate of zinc, alum (℥j to lbj of water), red or white precipitate (6 to 12 grains to ℥ij of lard), are of good service in acute eczema, or in chronic, when there is but slight infiltration, and the disease prevails only over a limited extent. When, however, there is considerable infiltration in chronic eczema these means do not suffice, and then the almost indispensable *schmierseife*, by reason of its slightly

caustic action, becomes the most preferable remedy. Its mode of employment varies according to the degree and extent of the eczema. When there is but slight infiltration, a rubbing with it once or twice a-day is sufficient; but when the infiltration is more considerable, a more frequent application is necessary, and even epithems composed of it may be required. When we have thoroughly rubbed the skin with the soap, until excoriations and red points have been produced, the surface should be washed and cold applications laid on until next rubbing. This procedure must be continued as long as the moisture and itching and the infiltration of the skin continue, and until the frictions no longer give rise to heat and excoriations of the skin. The soap must then be replaced by cold applications, and the treatment terminated by the employment of tar.

In many chronic cases, with great infiltration, the soap does not suffice, and we must have recourse to a stronger form of potash, viz., caustic potash, 1 drachm dissolved in 2 drachms of water. A pencil of charpie is dipped in this and well rubbed into the diseased parts for some minutes. These are then washed, and cold applications are kept constantly employed. One or two such cauterisations usually suffice, and when more are necessary they should not be repeated oftener than once a-week. Carefully applied, the caustic gives rise to no cicatrix, but the practitioner must himself always undertake its application. It is a very painful procedure, and is not often required. Cauterising with strong acids is to be avoided, as it causes great pain and gives rise to scars. The application of the nitrate of silver is of no avail. *Tar* is in its way just as useful a remedy as the soap, the time for its application being when the moisture and itching have ceased, and exfoliation has commenced, *i.e.*, when *eczema squamosum* is present. It may be used either alone or mixed with equal parts of cod-liver oil, and should be applied by means of a brush twice a-day, carefully avoiding washing the parts or allowing water to come into contact with them. As long as any redness or desquamation continues, the tar must be repeated. Sometimes, when the application of the tar has been premature, moisture and itching are observed at certain spots, and the preliminary treatment has then to be resorted to again. Some individuals cannot bear the tar at all, it giving rise to severe inflammatory action. An ointment of acetate of lead or oxide of zinc should in such cases be substituted. *Cod-liver oil* is a valuable external application, and by its aid alone we are able to cure the eczema when this has not lasted very long and the infiltration is not very considerable. It is also an excellent adjuvatory to the treatment by *schmierseife* and cold applications, as flannels soaked in the oil may be kept bound over the diseased parts during the night. Employed alone, the treatment is very tedious, and is objectionable on account of the disagreeable smell and befouling the linen which it gives rise to. Taken internally, it does not exert the slightest influence on eczema.—*Weiner Spitals-zeitung.—Med. Times and Gaz., Feb.* 16, 1861, *p.* 179.

69.—ON ALOPECIA AREATA.

By Dr. T. McCall Anderson, Fellow of the Faculty of Physicians and Surgeons of Glasgow.

[This disease is likewise called Tinea, or Porrigo decalvans; and by Cazenave, Vitiligo capitis. The parasite is the *Microsporon Audouini.*]

Alopecia areata is one of the most interesting, as well as the least understood, of the parasitic affections of the skin. A disease confined to those parts of the cutaneous envelope provided with hairs, it attacks principally the scalp; but the beard, the genital organs, and the hairy parts are also exposed to its ravages. Although not in any way dangerous to life, it is yet a rather alarming disease from the deformities which it may occasion; sometimes continuing to flourish till every particle of hair on the body is destroyed. The phenomena of this curious disease are best studied on the scalp. It consists essentially of the formation of rounded or oval patches of baldness, sometimes solitary, but more generally multiple. These patches commence, according to Willan, by the production of pustules, which are *very ephemeral.* They have, however, never been seen by other observers; and, indeed, it seems doubtful if Willan himself ever saw them.

On examining the hairs of the affected parts in the early stages they are found to be dull and lustreless, and more easily extracted than healthy ones. The skin from which they proceed is found to be slightly reddened, and it is also the seat of a peculiar swelling having the appearance as if the subcutaneous tissue was œdematous; but it does not pit on pressure, as in true œdema. At this early stage there is a whitish matter on the diseased skin and hairs, which is nothing else than the parasite which occasions the disease. It is not, however, in nearly such abundance as in Herpes tonsurans, a disease which it resembles in many respects. At the commencement itching is complained of, which is generally slight, so much so, indeed, as often to excite little attention, so that the disease is not observed, and continues its ravages till baldness has been produced. This first stage is of short duration, and is followed by the falling out of all the hairs of the affected parts, which takes place very rapidly, leaving a rounded bald patch. By this time the faint red tinge of the scalp has completely disappeared, being replaced, according to Bazin, by a condition exactly the opposite; that is to say, it has become perfectly white, having apparently lost every particle of its pigment. This white colour certainly contrasts strongly with that of the parts of the scalp provided with hairs; but it seems probable that this white colour of the bald patches is not so much owing to any defect of pigmentary secretion as to the loss of the roots of the hairs. After the hairs have fallen out they are replaced by a number of fine short downy ones, resembling the down on the cheeks of infants. By-and-by even this down disappears, the slight swelling of the subcutaneous tissue subsides, and we now see merely the rounded patches of

baldness, the skin of these parts being smooth and white. The disease is often very limited, there being only one small patch on the scalp; but more frequently there are several, and these, at first small, have a great tendency to increase and involve the neighbouring healthy parts by circumferential extension, so that at last a number coalesce, forming one large patch, having a serpentine form; hence the name ophiasis, sometimes applied to this affection. In this way the whole of the scalp may be attacked, and permanent baldness produced. Instances have been met with, as before observed, of the complete removal of the hair from the scalp, eyebrows, beard, and genital organs, so that not a single hair remained.

Treatment.—Let us suppose that we have a circular patch of alopecia areata on the scalp: the first thing to be done is to prevent it from extending. This is to be done by causing all the hairs within a couple of lines of the circumference of the patch to be carefully extracted. For those at the edge are very frequently, to a certain extent, impregnated with the parasite, though not so much as to render the depilation difficult; or, if not actually diseased, they are very apt to become so, as the affection spreads by additions to the circumference of the patches, and they are therefore much better away. The head, also, should be washed daily with black soap, as cleanliness is of great consequence in affections of the head, and the hair should not be kept too long. We must now apply ourselves to the diseased surface. All the downy hairs—if there are any, which is not invariably the case,—are to be removed; and this must be effected very carefully, according to the rules already laid down, for they are exceedingly friable and very apt to break off, leaving the diseased roots in their follicles. This depilation of the downy hairs must be persevered in and continued till the healthy hairs begin to make their appearance. Along with the depilation of the downy hairs, and of the hairs surrounding the diseased patch, a parasiticide lotion or ointment must be applied. One of those already alluded to (under tinea tonsurans) should be used daily, and probably the bichloride lotion, or alcohol, as recommended by Kuchenmeister, is the best. The whole head may also be occasionally washed with one of these solutions, so as to destroy any of the spores which may be entangled in the hairs but have not had time to take root. If the disease is far advanced, and no down is seen on the bald patches, the application of a blister may be made, as is practised by Jenner and Startin, and is sometimes useful, but it must be repeated to be successful. A common fly blister may be employed for this purpose, or the acetum cantharides or collodium vesicans. The acetum cantharides should be applied, on a sponge, to the part, and produces vesication in about ten minutes or a quarter of an hour. The collodium vesicans, a mixture of equal parts of collodium and ether cantharidalis, should be applied with a glass-brush, and two scruples are sufficient to blister a surface the size of the palm of the hand.

The vesicant which I am in the habit of using most frequently is a

strong solution of the bichloride of mercury (a drachm of the bichloride dissolved in seven drachms of alcohol). A piece of blotting-paper may be soaked in this solution, and applied to the part affected for five minutes; after which, cotton wad is substituted for it, and vesication is certain. There is no danger of salivation, as some might suppose.

Instead, however, of vesicating the affected parts, we may confine ourselves to the application of the more slowly acting parasiticide lotions or ointments, and to circumferential depilation; but very frequently, although we may thus prevent the disease from extending, we cannot cause the hairs to grow on the bald patch, for, when long left to itself, the parasite destroys the hair-follicles, and permanent alopecia is the result.

It is necessary, at the same time, as the disease is attacked locally, to attend to the general health, to correct any disorder of the system, if such exists, and to endeavour to change that state of system favourable to the growth of the parasite.—*Med Times and Gazette, March* 23, 1861, *pp.* 298, 300.

70.—*On Local Applications in Urticaria.*—I have found so much relief and such rapidly successful results from the local use of the liquor plumbi subacetatis in attacks of urticaria, that I wish to direct attention to it. The extreme uneasiness and irritation which this eruption causes in children is well known. Even adults suffer sometimes severely from it, and although from its evanescent character, it is not so frequently brought under medical notice as more persistent yet less distressing affections, still it happens that we are asked sufficiently frequently, what will best relieve the intolerable sensations that it produces. Of course I do not wish in any degree to undervalue the employment of such constitutional treatment by alkalies or aperients, as may seem desirable, or that in relieving the irritation caused by the eruption, our attention should be diverted from these numerous exciting causes of an attack, such as fruit (especially I think hazel nuts and fresh gooseberries,) fish, shell-fish, &c., and which, by producing gastric derangement, are distinctly connected with the appearance of the rash.

I have found a mixture of liquor plumbi in an undiluted state, mixed with one or two parts of glycerine to be most useful as a local application; the advantages of this mixture are considerable, for if we apply liquor plumbi alone to the skin, although it does speedily relieve the itching that is caused by the eruption, yet we find it is very difficult to wet the surface with it; in fact, it flows off the skin as if the part were oiled, and a repetition of the application becomes necessary; this is greatly obviated by combining glycerine with the liquor plumbi. I have now used this remedy for a long period with success, and I am not without hopes that it will likewise be found of benefit in many other cutaneous diseases; it would be worth trying on a large scale in erysipelas and small-pox, in neither of which affections have I used it as yet sufficiently.—*Dublin Hospital Gazette, Dec.* 1, 1860, *p.* 365.

71.—ON A CASE OF BLOODY SWEAT.

By Dr. THOMAS K. CHAMBERS, Physician to St. Mary's Hospital.

[The following is a most singular case—a most rare form of vicarious menstruation. The patient is described as short and stout, twenty-seven years old. She has suffered much from headache and dyspepsia. She has only menstruated twice, and that in consequence of the application of leeches to the uterus; and at or immediately after the last period (three and a half years since) the peculiar symptoms from which she suffered first appeared.]

She feels first a peculiar soreness and tenderness of an isolated spot, which enables her to predict that in the course of a few hours an eruption is going to commence. The first appearance of this is an erythematous blush, sometimes slightly raised above the surrounding surface, but not so much as in erysipelas. After an uncertain time, seldom more than a few hours, there may be detected a scattered crop of fine vesicles, like sudamina, mixed with a fine serous dew, uncovered by any pellicle. This never lasts long enough to form colourless drops, for quickly it becomes blood-stained, and then little points of blood are seen oozing out, sometimes so slowly as to dry and form a scab, sometimes collecting into great thick gouts, and trickling in a ghastly way down her face. The eruption runs through its stages quickest when she is in bed, and especially during sleep. Suspecting from the strangeness of this story that there was some collusion, I had the bed watched, and the nurse saw the blush come, and the blood ooze out in the manner described while the patient was fast asleep. If rubbed, washed with water, or otherwise interfered with, the bleeding is much increased and prolonged; but if left alone to dry in a scab, it stops in a week or ten days, usually, however, to be succeeded, before it is quite recovered, by a similar eruption in another place. Sometimes, at irregular periods, there was an interval of a week or a fortnight; sometimes the cutaneous phenomena were replaced by bleeding from the nose, sometimes by vomiting of blood, but never by hemorrhage from either lungs or bowels. These symptoms continued nine months, and were relieved by anticipating the eruption of blood, with leeches applied to the spot where it was expected. The discharge became serous, then was like little blisters, and finally ceased when her health was re-established by the sea air of Margate.

She continued well till last September, when she was admitted here as a mild case of erysipelas of the face. It really looked exactly like erysipelas; but it puzzled us a good deal when it began to bleed, and has continued to puzzle us ever since, as regards its nomenclature, pathology, and treatment. She was seldom seen free from cutaneous hemorrhage from that period till four weeks ago, but it has varied a good deal in violence. During this attack, the face has not been the only part affected. When she lies down much in the day, that, indeed is almost always the locality where it has appeared; but when

she is about, the legs and thighs have exhibited like appearances; both forearms, too, and once the chest, were attacked. The loss of blood, however, was less than on the brow. Examined under the microscope, the fluid exuding from the skin contained blood-discs in a natural state—blood-discs with roughened edges and shrunken, much granular matter, dark fatty-looking specks, and scales of epidermis. It did not coagulate into rolls. Blood drawn from a prick in the finger looked perfectly natural, and coagulated into rolls, leaving the usual number of pale globules free. She has twice thrown up from the stomach about half a pint of dark brownish purple sanguineous fluid, I and have occasionally seen her pocket-handkerchief stained with blood reported to have come from the nose. She states that she has been hysterical for the last year, and has had occasional paroxysms of crying and low spirits during her residence here, but with that exception there are no visible disorders of the bodily functions. The action of the bowels is free, the urine pale and sufficiently copious, the appetite very small, especially for meat. But in spite of the little she eats, the constant loss of blood by the disease, and a persistence in a very lowering treatment, she has got very fat—pale, it is true, and flabby, but decidedly fat,—and not weaker in muscular power than on admission. In the last month, during which her ailment has been declining, she has been less hysterical, and had somewhat more appetite. She tells me that a similar increase of flesh occurred during her last illness, three years ago. There is no leucorrhœa, and examination per vaginam fails to detect anything abnormal in the parts which can be explored with the finger. She complains of pain, and becomes hysterical during the process. The lungs and heart appear quite healthy.

This appears to me a case of cutaneous hemorrhage, or, in homely Saxon, "bloody sweat," vicarious to suppressed menstruation. Some physicians have fancifully attributed to suppressed menstruation every kind of increased secretion or congestion which accompanies the derangement of uterine function, and the female public are still more disposed to endorse this imaginative pathology. I am far from agreeing with them, and would restrict the explanation to those cases where a flow of blood from a distant part not organically diseased follows (*follows*, not precedes) a defective condition of catamenia, and where this flow of blood is a relief to previous symptoms, and not weakening to the patient. The most frequent forms under which this vicarious menstruation occurs are, hæmatemesis, epitaxis, hæmoptysis, and hemorrhoidal flux, instances of which you may fairly expect to see in the hospital during your pupilage; but it is a chance if any of us see again a case where the discharge occurs through the external skin, and it behoves us therefore to seize the opportunity of taking note of the phenomena. My opinion of its rarity is formed on the difficulty I have experienced in finding records in authors who write on allied subjects. I suppose, since the invention of printing very few exam-

ples of so strange a thing can have escaped being put into type; yet Haller, in his exhaustive work, can only cite eighteen from the industrious collectors of physiological curiosities during a century and a half. After him, Van Swieten ("Commentaries on Boerhaave," sec. 1286) quotes a case from Boerhaave's MS. remains, of a young girl who from her twelfth year menstruated through an eruption of pustules in various parts of the surface, which pustules, after the menstruation, entirely disappeared. Pinel, in his "Nosographie Philosophique" in 1802, repeats Haller's collection, but evidently cannot then cap the cases with any later ones at all to our purpose. In Hufeland's "Journal der Practischen Arzneykunde," there is mentioned a woman who menstruated through a scar of an abscess at the navel at her proper periods for two years; and a girl is said to have told a Mr. Gutgesell that her menses always appeared through a red spot over her knee. In the "Archives Générales de Médicine," an. 1829, (t. xix., pp. 212 and 236), are two cases recorded: one of a young lady, who, after ten years' suppression, menstruated for three years through a vesicular eruption in one finger; a second, of a prostitute, in whom the discharge occurred through spots of the size of a five-franc piece, which appeared from time to time one after another on the breast, in the axilla, on the back, the buttocks, and the epigastrium. This is called by the term I have to-day used, "bloody-sweat," and the description accords closely with that of our patient; especially in the eruption being less periodical and more continuous than happens in most vicarious menstruations. The uterus also was healthy, for she became pregnant and bore a child.

In 1836 I find a rather fuller account of a case related in "Schmidt's Jahrbucher," by Heusinger. The woman had diseased ovaries and recto-vesico-vaginal fistulæ, and though sometimes the catamenia appeared at the proper place, they were generally arrested there, and appeared in a variety of parts of the external skin. The most common part was, as with our patient here, the face. She had suffered five years, and been in several hospitals. Heusinger says that she was so hysterical that she would have made a capital subject for mesmerism: whether he means that he suspected her of imposture I cannot say. M. Lheretier in his Traité Complet des Maladies de la Femme, (Paris, 1838,) cannot add any case from his own knowledge, but he quotes one recorded by M. Gardien, as reported by M. Brulé, of a young girl with suppressed catamenia, who had periodical monthly hemorrhages from vesicles in the legs for six months, from an eruption on the left arm for a year, through a sore on the thumb for six months, by the upper eyelid for two years. This last was preceded by erysipelas, as was also an eruption of the same nature by the navel. She had also hæmatemesis and other vicarious hemorrhage not to the present purpose. Curiously enough, this identical case is again repeated by M. Brierre de Boismont, and said to be quoted from Pinel's Medicine Pratique, but I cannot find it in that author's works at all.

It is doubtless our old friend of Lheretier, who also again turns up in Meisner's "Diseases of Women," and there gets made into two persons by having her history quoted separately from each author. The last-named learned and valuable work adds no instances from contemporary observation of menstruation by the healthy skin; and I believe the same may be said of more recent authors. The chatty Meigs and the judicious West do not seem to have seen it.

The literature of the subject is scanty simply because the number of patients is scanty: and I cannot say I have learned anything new from books which this one case had not already taught, though they have given me the satisfaction of agreeing in almost all points of doubt. With their help, and the case before us, I think the following conclusions may be arrived at:—

1. Cutaneous menstruation occurs in robust and healthy-looking, rather than in anæmic persons. It appears, in fact, to be a plethora.

2. It may occur without any detectable disease of the parts of generation; instance this girl, the prostitute mentioned in the Archives, who had a child, and Mr. Gutgesell's case who was going to be married.

3. But may also be caused by disease in those parts.

4. The catamenia are not necessarily entirely diverted from their usual channel.

5. Periodical cutaneous hemorrhage is not necessarily connected with the catamenia at all. Thus Van Swieten quotes from Beneventus the case of a man, who once a month sweated a great quantity of blood from the healthy skin of his right flank.

6. It is not always strictly periodical.

7. *Suppressio mensium* may go on for many years without any tendency to vicarious menstruation. Of this you see frequent instances in cancerous disease of the uterus; and a remarkable one in a female, now under my care in Victoria ward, who never had a womb at all, and in consequence has never menstruated either naturally or vicariously.

8. Cutaneous menstruation usually appears in the shape of a slightly swelled erythema, painful and tender, on which vesicles form; the vesicles quickly burst, and there is poured forth a sero-sanguinolent exudation, which then becomes bloody, and continues to exude like a bloody sweat for various times, from four days to a fortnight. No scar is left, but the skin is slightly discoloured for some time afterwards in the parts affected.

9. Hence I do not know if we ought to call it a disease of the womb any more than a disease of the skin.

10. Bloodletting is the most efficacious treatment; and it is most efficacious in the form in which it is known to produce its most powerful physiological effect—namely, in small repeated doses, and as close as possible to the seat of action—that is, on the spots affected. The measure of the good effect of the bloodletting is the relief experienced by the patient without loss of strength.

11. No other remedies are as yet known to be of any advantage.—*Lancet, March* 2, 1861, *p.* 207.

72.—ON OPENING DEEP ABSCESSES.

By JOHN HILTON, Esq., F.R.S., Surgeon to Guy's Hospital.

[The following extract is from a course of lectures delivered by Mr. Hilton in the Theatre of the Royal College of Surgeons. The question as to what is the best and safest method of opening a deep abscess is one of the greatest importance.]

Persons have died, and many lives have been endangered, by hemorrhage consequent upon opening a deep abscess by the lancet or bistoury; and within the last twelve months I have known two lives placed in great jeopardy by the use of the lancet to open deep abscesses. These circumstances alone will, I hope, be sufficient excuse for my pressing this subject on your indulgent attention; and my own long experience of its value justifies the high opinion I entertain of the method of opening a deep abscess which I am about to recommend to your notice as the safest and best. It is safest, because it is scarcely possible to inflict by it any injury on bloodvessels or nerves. I think it is the best plan too; for if the deep opening into an abscess be lacerated and bruised, it is not at all likely to close by adhesion; and undoubtedly, when we have opened a deep abscess, we are desirous that the deep opening should not close immediately; and in principle, all will admit that if a lacerated opening be made instead of an incised one, that will be one of the circumstances adverse to a quick re-adhesion. Nay, more: supposing a lacerated wound to be made deep in the thigh, for example, and a bloodvessel divided by being torn asunder, it would be a lacerated wound of the bloodvessel, and therefore not so likely to bleed as an incised wound.

Some surgeons, when speaking of deep abscesses, say, with rash confidence, "Plunge in a knife." It is a grand term—"Plunge a knife into a deep abscess." It is an heroic, not a courageous plunge for the surgeon; for it is without danger to himself, but perhaps it is a fatal stab to the patient. Some more careful surgeons say, "Wait until the abscess comes nearer to the surface, so that it can be opened without danger;"—but the patient may die in the meantime. Now the plan I have been in the habit of adopting and recommending is this—for example, in the case of opening a deep abscess in the axilla,—cut with a lancet through the skin and cellular tissue and fascia of the axilla about half or three-quarters of an inch behind the axillary edge of the great pectoral muscle; at that part we can meet with no large bloodvessel. There is only a small branch of one of the external thoracic arteries, which sometimes runs along the edge of the axilla; excluding that, which if wounded can be easily ligatured, we run no other risk, so far as I can see. Then push a grooved probe or grooved director upwards into the swelling in the axilla; and if you will watch the groove in the probe or director as it is being passed up through the comparatively healthy tissues into the axilla, a little stream of opaque serum or pus will show itself. Then take a blunt (not a sharp)

instrument, such as a pair of "dressing forceps," and run the closed blades along the groove in the probe or director into the swelling, and by opening the handles you at the same time open the blades situated within the abscess, and so tear open the abscess; and, lastly, by keeping the blades of the forceps open during the withdrawal of the instrument, you leave a lacerated track or canal, communicating with the collection of pus, which will not readily unite, and which will permit the easy exit of the matter. In this way you may open an abscess deep in the axilla, or in other important parts of the body, without fear of inflicting any injury upon the patient. Having been connected many years with a large hospital, I have necessarily had good opportunities of trying this method. I have not opened a deep abscess during that time in any other way, and I can say, honestly and truly, that it has never failed, and that I have never observed any inconvenience from it.—*Lancet, Nov.* 10, 1860, *p.* 454.

73.—ON THE INFLUENCE OF REST IN THE CURE OF ABSCESSES, SINUSES, AND ULCERS.

By JOHN HILTON, Esq., F.R.S., Surgeon to Guy's Hospital.

[Much of the difficulty experienced in curing abscesses, and sinuses leading from them, lies in the frequent motion to which many are subject, either by adjacent muscles, or by fascia covering them. For there is probably no fascia in the body which is not influenced by some muscle. Physiologically and surgically considered, fascia is the insertion of muscle.]

Sub-occipital Abscesses.—Abscesses under the occipito-frontalis muscle are sometimes very large, and resist surgical treatment for a very long period. Why is this? Let me remind you that the areolar tissue in which this abscess exists is abundant, and that the whole superficies or dome of the abscess is under the influence of the occipito-frontalis muscle. Hence there is no rest to the abscess; it never has a chance of quietude, or of accurate and persistent coaptation of its surfaces, from the disturbance produced by the movements of this muscle. That such abscesses are difficult to heal will be admitted when I mention the particulars of some cases; and I will endeavour to demonstrate to you that in the surgical treatment of such cases the great object should be to keep the occipito-frontalis perfectly quiet. I would say, then, that chronic abscesses or sinuses under the scalp may be cured by keeping the occipito-frontalis quiet and at rest by strapping.

In illustration, I may mention this case to you. Last spring I was requested to see a stout gentleman, aged fifty, who had fallen down the hold of a ship. He had been taken to the London Hospital, where his wound was well dressed. The anterior half of the scalp was injured; it had been turned forwards and downwards, quite

over his face, by the accident. This flap was replaced in accurate position, bandaged, and strapped, and the patient was sent home. Not any primary adhesion took place in the wound, and in a few days his surgeon thought it right to take off the dressing, when it was found that the scalp itself was much swollen, and that the whole of it was lifted up or raised from the bones by suboccipital suppuration. The patient had subsequently two severe attacks of hemorrhage, from sloughing and ulceration of the temporal arteries, which required ligatures; and it is worthy of remark that the bones of the cranium were denuded of pericranium to the extent of several inches, and were daily exposed in this condition during nearly a fortnight. Yet the bones did not die, their nutrition being derived from the blood supplied by the arteries of the dura mater. The wound was dressed daily with lint and warm water, and the pus squeezed out from under the whole of the occipito-frontalis; but the abscess would not heal, and the question was, how to get the pus from under the posterior part of the scalp—for there was no outlet for it in that direction—and how to secure rest to the movable dome of the abscess. The hair upon his scalp was shaved off, and long strips of plaster were so arranged across and around the head as to empty the abscess, to keep the occipito-frontalis quiet, and to press it downwards upon the pericranium and bones: this was done simply for the purpose of giving rest to the parts. The rapidity with which the abscess then healed was very remarkably in contrast with the tardy results of previous daily dressing and emptying the abscess. He quickly got well, and without any necrosis of bone.

Cervical Abscess under the Platysma Myoides.—Abscesses in the neck, under the platysma myoides, are well worthy of your attention. When these abscesses are opened either by the surgeon or by nature they are sometimes very difficult to heal; and I believe the great difficulty depends upon the fact that they are under the influence of the platysma myoides. About two months ago, my friend, Dr. Daldy, brought to me the son of a clergyman, who was suffering from a large ulceration, the sequel to an abscess, lying deep in the neck. He had had the advantage of good professional attendance and general treatment; but the ulceration not only did not heal, but was slowly extending. We determined to see the effect of rest to the ulcer. Strips of plaster were placed across the neck, and a layer of cotton wool over the plaster and the ulcer, upon the side of the neck; these means, aided by gentle bandaging, kept the platysma myoides quiet. The ulceration had continued a considerable period before I saw the patient, going on from bad to worse; but immediately on this plan being carried out, the surface of the ulceration began to improve in appearance and to diminish in size, and in a very short time was perfectly well; although the child was not, upon the whole, a very healthy subject.

Deep Cervical Abscess, followed by a Sinus.—I have here the notes of another case, where a sinus existed in the neck, and was cured by

rest. This case was of two years' duration, and was cured in three weeks by rest. I am almost afraid you may be induced to fancy I am using the language of exaggeration in this statement; but really it is not so.

In 1849, Elizabeth H——, aged twenty, living at Lambeth, had had a large abscess extending deeply under the platysma myoides and the sterno-cleido-mastoideus. This had continued as a long sinus, discharging freely during many months. She had been an hospital out-patient during two years. Iodine, &c., had been injected into the sinus, and a seton passed through it, and retained within it for some time without any benefit. Strips of adhesive plaster were drawn tightly over and across the muscles covering the sinus, and the head was steadied by a pasteboard splint, cut rudely into a form which could be adapted to the pelvis, along the back of the shoulders to the back of the head, and then laterally on each side, so as to embrace the whole of the head in a circle of pasteboard.* A figure-of-8 bandage was then applied over this apparatus around the head and under the axilla, crossing in front of the chest, so as to fix the head and neck forward and a little downwards firmly upon the shoulders, and to relax the muscles. The sinus ceased to discharge at the end of a fortnight; but she continued to wear the apparatus for a short time—about three or four week—longer, as a further security.

This patient was thus cured by rest.

Small Superficial Cervical Abscess.—We are often troubled to deal satisfactorily with the results of small superficial abscesses in the neck. Such open abscesses under the skin are mostly said to be scrofulous; but I suspect their persistence depends not unfrequently upon their close relation to the platysma myoides. I believe—and I am speaking here from experience—that by placing a pad of cotton wool, twice as large as the area of the disease, over the part, and by fixing it there so as to keep the platysma myoides and all the surrounding parts at rest, many of these cases that seem to defy almost every kind of treatment may be successfully dealt with. I have had opportunities of observing the success of this treatment in very many such cases.

Cases of Carbuncle, followed by Sloughing.—We all know that it is not easy to manage successfully the treatment of a patient who has had a large carbuncle on the back of the neck near the scalp, which, by destroying the subcutaneous areolar and fascial structures, leaves large portions of loose overlapping skin, blue, dark coloured, and congested, showing a very feeble power, and, added to this condition, the trapezii muscles exposed to view. Now I wish to show the therapeutical value of local rest in the treatment of such a case.

Two years ago I saw the wife of a physician whose condition accura-

* This splint was made off-hand with wet soft, thick pasteboard, covered by linen, pressed upon the surface of the body, so as to become moulded to it, and then allowed to dry firm in its position by the bandaging.

tely resembled that which I have just delineated. She had been previously attended by a very eminent London surgeon. The case was not proceeding satisfactorily; there was no local evidence of repair; the wound had remained stationary during some time before my visit. On looking at the patient's neck, it appeared to me that there were two additional requisites in the treatment which might help the cure: one was to arrange some simple mechanism which would keep the trapezii muscles quiet; and the other, to support in their proper positions, and to maintain in a state of perfect rest, the loose, feeble flaps of skin. I hoped by such means to facilitate adhesion of the two granulating surfaces. The loose flaps of skin were laid neatly upon the subjacent trapezii, and then a large, thick pad of cotton wool was firmly fixed upon the surface of the flaps of skin and surrounding parts. A bandage was applied around the head, and extended as a figure-of-8 bandage, crossing behind the neck and under the armpits, in order to fix the head, neck, and shoulders, and control the trapezii. In twenty-four hours the healing commenced, and proceeded under the same local treatment, to the most satisfactory and speedy termination.

Not long after that time, I saw a patient with Mr. Wright in the Clapham-road, where precisely the same circumstances occurred, with precisely the same treatment, and precisely the same results. Here the parts were kept at rest partly by a thick pad of cotton wool pressing upon the flaps, and by means of a bandage to keep the head and the trapezii muscles in a state of rest. Cases of the same kind, with the same result from the same treatment, have occurred to me lately in Guy's Hospital.

Popliteal Abscess and Sinuses.—Sinuses in the popliteal region, in unhealthy subjects, are very difficult to cure, except by local rest; and here I will take the liberty of reading part of a note which I received from a surgeon in Essex, who had sinuses at the back of his knee-joint:—

"Jan. 9th, 1853.—My sinuses occurred after an abscess in the popliteal space, which left a very irritable, unhealthy ulcer, the size of a walnut; and several long and deep sinuses extending from it, amongst the tendons of the hamstring muscles. Not being able myself to improve this ulcer, I consulted the late Sir. A. Cooper, who stated that it was owing to a defective state of my general health, and ordered me steel and quinine, to inject nitric acid lotion, to take exercise, and to wear a high-heeled shoe. At the end of many weeks, the ulcer and sinuses remained as they were. You then saw me, and ordered my leg to be flexed, and placed upon a resting splint upon a wooden leg, and moderate pressure upon the sinuses with soap plaster. I continued this plan, and in two months I was quite well, and have been so ever since, and am in active occupation in my practice."

Small Facial Abscess.—In some small abscesses sometimes occurring in the face, cheek, or neck, which have opened by a minute ulcer-

ation through the skin, the aperture communicating with the sac of the abscess, I have found the best result from carrying out this principle of rest to the part. I have merely passed a small probe into the aperture in the skin for the purpose of keeping it open whilst collodion was being applied upon the dome of the abscess, so as to press the dome upon the floor of the abscess. In this way pressure and rest are secured, and by coaptation the parts are kept undisturbed; the process of healing then commences, and I think the patient gets well more quickly by this than by any other local method of treatment.

Sinuses, then, are cured by the adoption of local rest; and I think that if surgeons, instead of affixing to every sinus the opprobrium of bad general health, would take into consideration the possibility of there being some special local disturbing cause associated with it, such as muscular or other movements, more success would result from a very simple plan of treatment by rest.

[The curative agency of rest in painful ulcer, is well known, it is an exemplification of what Mr. Hilton calls "physiological rest."]

Painful and Irritable Ulcers.—An irritable ulcer is to be distinguished from an inflamed ulcer, no doubt, by the quantity of lymph which is poured out upon the inflamed ulcer, and the high degree of its temperature. Irritable ulcers, as we know, are exceedingly painful, and sometimes very difficult to cure. As far as I have been able to detect their real essential pathological character, it depends upon the exposure of a nerve on the surface of the ulcer. Of course it is quite clear that every ulcer must have nerves more or less exposed on the surface; but in the case of an irritable ulcer, it seems probable that the sheath of the nerve is destroyed, and that the end of the true nerve-fibre or tubule remains denuded, and so causes the exquisitely sensitive and painful character of the ulcer. Upon this exposure of nerve depends, I believe, the chief feature of what we term an irritable ulcer, and the method of detecting the precise seat of the exposed nerve is very simple. You must understand that my remarks on this subject are founded on an experience in such cases of twelve or fourteen years. Having a very painful uninflamed ulcer before you, you apply the blunt end of a common probe upon the sore, and as you move it about, presently the patient exclaims, "Oh! there you hurt me dreadfully." Then you go a little further on with the same method of examination, and scarcely any pain is felt; but on coming back to the same spot, the patient is again "dreadfully hurt." At that painful spot some small nerve is exposed on the surface of the ulcer. The first time I observed this fact, and placed upon it what I believe to be the right interpretation of it, was in a case of a patient who had a thecal bursa extending from the palm of the hand under the annular ligaments to above the wrist. I made an aperture into the bursa above the wrist large enough to let out all the fluid and solid contents,

and the bursal swelling gave me no special trouble, and it was speedily cured; but at the seat of the opening a very irritable painful ulcer remained which I could not cure by any local application. On examining the ulcer with a probe I discovered a point of exquisite tenderness. The normal position of the small palmar branch of the median nerve corresponded with the site of the local sensitiveness and seemed to explain the pain. I then passed a small-pointed bistoury under the track of the nerve above the tender spot, so as to divide the nerve between the spinal marrow and the irritable point of the ulcer; from that time the pain ceased, and the ulcer rapidly healed. It was cured by the "physiological rest" resulting from the division of that little nerve.

Painful Granulations following Injury.—Some long time after the occurrence of this case, I had a patient in Guy's Hospital suffering greatly from an ulcer at the end of the finger, in which it was supposed there was a piece of broken glass, as the original injury was a cut from breaking a window. She had been under the observation of a surgeon, who had tried repeatedly to get out the supposed piece of glass, and had punished her severely, but unintentionally. She came into the hospital, and I thought the case would give me a good opportunity of making a demonstration of what I had long deemed to be correct, and taught. On placing the broad end of my own finger upon the ulcer, it gave her exquisite pain; the broad surface of my finger, however, was not a sufficiently accurate localizer of the pain. I then employed the rounded end of a probe, and with great care examined the whole surface of the ulcer by pressure, until I came upon a spot that was exquisitely tender, and produced dreadful pain to the patient. With a pair of scissors I cut out the painful granulations. Explaining to Dr. Habershon, the Demonstrator of Anatomy at Guy's Hospital, the views I entertained regarding the cause of the painful granulations, I requested him to examine them by the aid of the microscope, and he found in them, and near the surface, as I had expected, looped filaments of nerves, thus completing the demonstration of the cause of the pain. From the time of my cutting away those sensitive granulations the pain ceased, and the sore began to heal; there was no more trouble or difficulty as regards the treatment of the ulcer; it got well by giving it "*physiological rest.*"

Exquisitely painful Ulcer after Injury.—About a year and a half ago, I was requested to see a gentleman's coachman, who, on getting off his box seat, slipped his fingers between the lower bar and his seat, and thus had two of his fingers broken off at the second phalanges. One of them went on rapidly towards healing and healed very well. The other remained swollen, irritable, very painful to the touch or on exposure to the air, preventing sleep, and producing great constitutional disturbance. We failed to relieve these symptoms by the local and internal employment of opium. This unhealthy condition could not be from any constitutional defect, because one finger did well; nor

could it be from the result of any dissimilarity of the original injury, for they were precisely alike. With the surgeon in attendance, I made a careful examination of the part, and when I placed the end of a probe towards the edge of the ulcer upon the finger, it detected a spot which was exquisitely tender, and the patient screamed out—"Oh, pray, for God's sake, cover it over, I can't stand it." The position of this pain was in the course of one of the lacerated digital nerves. I passed a pointed bistoury under the nerve, about one-fourth of an inch above its exposed portion upon the wound, and so divided the nerve. The pain in the ulcer immediately ceased, and the touch of the probe caused no uneasiness. From that time all the local symptoms rapidly improved, and the case gave no further trouble, being quickly cured by "physiological rest."

These cases prove distinctly that an ulcer may be very much modified in its character from the exposure of a nerve in the wound.

I mentioned this subject to my colleague, Mr. Cock, some time ago, and shortly afterwards he had an opportunity of testing the value of the observation. He recognised the condition of such an ulcer as that I have referred to, and divided the exposed nerve; the patient lost the pain, and the ulcer quickly assumed a healthy character and got well.

Painful irritable Ulcer of the Leg.—I have here short notes of two other cases that have occurred lately, under my care, at Guy's Hospital, to which I will, with your permission, now allude.

John J——, aged 27. sailor; admitted on the 9th of November, 1859, suffering from syphilitic sore on frænum and penis, with secondary eruption, and a very painful ulcer on the inner malleolus of his left leg. The syphilis was treated and cured by Plummer's pill, five grains, twice a day; but the painful ulcer remained uninfluenced by the mercury.

This is the history of the ulcer provided by my dresser:—

"When leaping, about four years ago, he sprained his ankle, and an ulcer formed on the inner malleolus of the left leg. It had made frequent efforts at healing, but never cicatrized completely. It now looked irritable, with no inclination to heal, and was very painful, with intense nervous sensibility localized at its upper margin, which was ascertained by examining it with a probe.

"Jan. 24th, 1860.—Mr. Hilton passed a pointed bistoury a little distance above the tender spot under, and then through, the granulations, thereby severing the filaments of the nerves supplying the morbidly sensitive granulations; and although the patient made much ado about the operation, yet he immediately acknowledged himself relieved by it. The ulcer readily assumed a healing aspect, sensation over the other parts of the ulcer was not more acute than normal, the surface became covered by healthy purulent exudation, cicatrization daily advanced, and the ulcer was closed in a fortnight, and remained so until he left the hospital, on the 8th of March, 1860."

This was a case of irritable ulcer, cured by division of the nerve.

The other case may be put before you in a few words:—

Jan. 11th, 1860.—The painful spot of an old irritable ulcer was examined by a probe; the nerve supplying the tender granulations was divided; marked relief was the immediate consequence. This division of the nerve was done on the 11th January, and on the 16th this is the dresser's report:—

"The ulcer above mentioned is free from pain, and has assumed a healthy character; its edges are throwing new skin over the granulations. From this time, the painful ulcer required no special attention, and in ten days all was healed."

These are cases that appear to me to display very accurately the therapeutic value of what we may fairly term "physiological rest," to the abnormal sensibility of the surface of the sore. The division of the nerve had its effect upon the neighbourhood *physiologically*, and the ulcer began to heal.

I trust I may have thus briefly succeeded in pointing out the true pathological feature of what is termed a "painful irritable ulcer."—*Lancet, Nov.* 17, 1860, *p.* 477.

74.—ON THE SURGICAL TREATMENT OF SINUSES.

By Walter Jessop, Esq., Surgeon to the General Hospital and Dispensary, Cheltenham.

[The patient had on three occasions, suffered from bubo in the right groin, and the whole groin was radiated in every direction with sinuses, some running to a considerable depth.]

On seeing him in December, 1858, eight distinct sinuses could be traced. For nearly six months he had been under treatment; injections, setons, rest, and pressure had entirely failed to heal a single sinus. The integument was much indurated and discoloured, and the patient's health greatly impaired. Directing a generous diet, I determined upon a renewal of rest and a careful adaptation of pressure; confining my patient to bed, the better to insure the application of well-regulated bandages. For nearly a month this treatment was scrupulously carried out, but with no good effect. Setons of silk were then tried, saturated in solutions of chloride of zinc, iodine, nitrate of silver, and such like stimulants. They simply increased suppuration. On removing them, pressure was again resorted to, but equally without effect, although perseveringly applied for some little time. In one length of the principal sinus, through which it was impossible from its irregular nature to pass the full length of seton, it was necessary to cut down upon the probe and draw out the silk, leaving a further portion of the sinus untouched. To my surprise, this was the only part that had healed, a portion rather more than an inch in length. Arguing from this circumstance that if the

continuity of each sinus were destroyed at certain intervals of its course, adhesive inflammation might be more readily induced, I proposed to my patient that he should submit to a trial of the experiment, and to this he willingly consented.

Passing a probe into each sinus a distance of an inch, or an inch and a half, I cut or nicked down on its point with a small lancet-shaped scalpel, so destroying the continuity of the canal at that spot. Withdrawing the probe, I repassed it from the point of incision for another inch, repeating the proceeding, and so on along the track of each sinus. In this way numerous small incisions were made. Pressure was carefully re-applied over the affected parts. Very little disturbance ensued, but the patient complained of more life and soreness in the groin than he had done at any other period of his case.

Pressure was retained for five or six days, and upon examination it was evident that adhesive inflammation had been set up, and that there was a fair probability of a permanent cure. In less than a fortnight the major portion of the sinuses had quite healed. Those which had failed were again treated in a similar manner, and the second operation proved entirely successful.

I saw this patient at intervals for nearly six months after convalescence, and on several occasions carefully examined him. There was no symptom of a sinus remaining, and the thickened and discoloured integument was much improved in appearance and character; the patient's health, also, in every way was re-established.

T. M——, a mate in one of the Australian line of clipper ships, consulted me in February, 1860. Two long irregular sinuses had existed in the left groin for upwards of seven months; and a pustular eruption spread over the scrotum and thigh, evidently occasioned by the irritative nature of the secretion from the sinuses. His case was treated in a similar manner to the preceding one. The single operation was sufficient, and, under pressure, the whole had healed *in* twelve days. This patient I have not seen since.

The proceeding referred to in these cases is simple, effectual, and really productive of very little suffering. The sinuses were of so extensive a character, and involved so wide a surface of unhealthy integument, as to preclude all appeal to the vigorous use of the bistoury, or the application of the heated platinum wire—an admirable plan introduced by Mr. Marshall, of University College Hospital, and published in the "Medico-Chirurgical Transactions" for 1851. Pressure and rest, and careful attention to the general health, had been tried, over and over again, without avail; and not the slightest progress was made until the continuity of each fistulous track was destroyed at intervals, and in the manner above-mentioned.—*Lancet, March* 9, 1861, *p.* 235.

75.—*In-growing Toe-nail.*—The treatment is by dropping a few drops of melted tallow, made very hot, upon the diseased part. In the Medical and Surgical Reporter for October 13th, Dr. H. M. Clarkson reports a bad case of in-growing nail at once relieved, and soon cured by one application of this simple remedy.—*Amer. Med. Monthly, Dec.* 1860, *p.* 439.

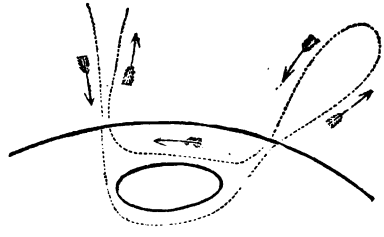
76.—THE SUBCUTANEOUS APPLICATION OF THE METALLIC LIGATURE TO THE CURE OF VARICOSE VEINS OF THE LEG.

By Dr. R. J. Levis.

Dr. Levis thus describes his operation:—To perform the operation it is important that the veins be distended, so as well to determine their outlines and avoid any risk of wounding them. This may be accomplished by putting a band around the limb above the knee, while the patient is in the erect position; or this may be rendered unnecessary by performing the operation whilst the patient continues standing. If the latter position be chosen, the patient, in order to have the leg at a convenient height, stands on a chair or table which is placed by a wall, against which he steadies himself. The points chosen for ligature are, first, the trunk of the saphena, at the highest point where there is evidence that, owing to the abnormal dilatation, its valvular structure is imperfect; then the largest and most superficial veins, at places where they are most isolated; and, finally, those in the neighbourhood of ulcers or eczematous eruptions of the integument. The only instruments and appliances essential are a long straight needle, some silver or iron wire, adhesive or isinglass plaster, and a roller bandage. The needle should be straight, and two and a half or three inches in length, and differing from the common surgical needle in having a sharp, round point, *which perforates without cutting.* In the absence of a needle properly adapted for carrying the wire ligature, an ordinary fine darning needle will be quite suitable. Experience has proved to me that a straight needle is much more manageable for directing the point accurately than the usual curved one, and the policy of avoiding the cutting or spear point, where, as in the varicose state, even the innumerable venous capillaries are often in a varicose condition, and which, when wounded, pour out blood profusely into the cellular tissue, is obvious. The wire had better be of a fine gauge, as number 30, or finer. Pieces of adhesive, or isinglass plaster, the latter preferred, one or two inches square, are useful. The bandage may be six or eight yards in length, and two and a half inches wide. A wire-twister is a convenience but not an essential to the operation. The patient being in a proper position, the operation is commenced by feeling for the edge of the vein to be ligatured, and entering the needle perpendicularly until a point beneath the under surface of the vein is reached. Then the shaft of the needle is

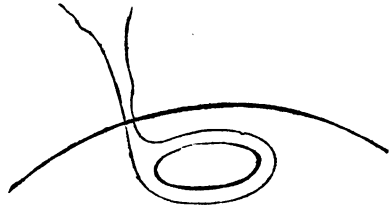
depressed and its point pushed horizontally beneath the vein *until it* makes its exit through the integument on the opposite side of the vein. The exit is facilitated by pressing on the integument with the fingers of the left hand over the point of the needle. After the needle is withdrawn, leaving a wire beneath the vein, it is *re-entered at the same orifice*, but this time passes *above the vein*, traversing the space between the integument and the vein, and makes its exit at *the point of original entrance.* A slight pulling on the wire draws beneath the skin the loop of wire left on the opposite side of the vein, and all that is seen of the wire is its two ends projecting from the same orifice. Thus the vein is surrounded by a single wire. Proper care will avoid a risk of wounding the vein, but if there should be evidence that this has occurred, the needle ought to be at once withdrawn and another point for the operation selected. The accompanying outlines, to make the matter more explicit, illustrate the course of the needle and wire, and the manner in which the wire encircles the vein.

The wires are then pulled sufficiently tight to simply constrict the vein, approximate its sides, and stop the circulation through its calibre. The object, be it understood, is not to induce a rapid



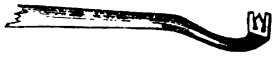
Course of the needle.

ulceration through the coats of the vein. In this, my own practice differs from the directions generally given for the ligature of varicose veins, and on this peculiarity, perhaps, depends its apparently absolute safety. It is true that to insure a perfect closure of a varix it is necessary that it should be actually divided, but this division is best



Wire surrounding the vein before being tightened.

accomplished slowly, and only after the slight pressure on its walls has excited within a plastic exudation which agglutinates them. Exposure of an opened vein to a pyogenic surface is in this way with certainty avoided. The pressure made on the vein by pulling the ends of the wire is secured by twisting them. This may be done by means of the fingers, forceps, or the wire twister. A simple form of the latter instrument, which I prefer, being easily placed on the wires without threading through holes, as in the form of instrument generally used, is herewith represented.



Wire Twister.

The wire is finally cut off, leaving an inch or more, which is laid flat upon the skin; the place of operation is covered with a small piece of adhesive or isinglass plaster, and a roller bandage envelopes the limb up to the knee. If an ulcer exists on the limb, it is to be simply covered, previously to the application of the bandage, with several thicknesses of dry lint. The patient rests horizontally, without disturbance of the dressing, for ten or twelve days. After this time all dressing is removed, and traction is daily made on the wires to accelerate their removal. But little more confinement of the patient is usually requisite. Decided relief from swelling of the varices is experienced, and he is frequently able to walk about with comfort long before the wires are removed. In one of my cases, a labourer had two wires remain in his leg for eight weeks, during the latter part of which time he continued his occupation with but little inconvenience; in another case the ligatures, six in number, were all spontaneously removed on the fourteenth day. On removal of the bandage on the leg, the ulcer, if dependent on the varicose condition, usually seems to be really *dried up*, and cicatrization rapidly follows.—*American Med. Times, Dec.* 15, 1860, *p.* 428.

DISEASES OF THE EYE AND EAR.

77.—ON IRIDECTOMY.

[The surgeons of the University of Bonn consider that the operation of Iridectomy is equally beneficial not only in acute and chronic glaucoma, but in acute choroiditis, in ulceration of the cornea connected with iritis, and in several other rapidly progressing and destructive affections.]

Professor Weber has, in most of his cases, only resorted to iridectomy after the antiphlogistic remedies, and especially the administration of calomel, even to salivation, had failed; and the success has exceeded

his expectations. The process of operation, although simple, nevertheless, requires great care. The incision should be an ample one, directed superiorly and internally, beyond the margin of the cornea, and nearly to the level of the iris, so that the ligamentum pectinatum iridis is severed. If there are no adhesions of the iris, it generally prolapses immediately, and there is no difficulty in seizing it. The segment of the iris should be rather large, and Mr. Hancock is quite in error when he says in his paper on division of the ciliary muscle in glaucoma, that all oculists agree that the smaller the quantity of iris removed, the better. On the contrary, small and incomplete excisions of the iris are rarely attended by striking success; and it is, perhaps, to this circumstance that the failures of some English oculists, who have only cut out infinitesimal portions of the iris, are to be ascribed. The segment of the iris should be completely removed up to the ciliary margin of the iris. Cutting of the cornea must be carefully avoided, as, if this be done, it is not possible to remove the iris up to the ciliary margin. Dr. A. von Graefe considers that the removal of the tension of the bulbus is the cause of the curative effect; and it is certainly true that, in the morbid conditions above mentioned, the intra-ocular pressure is so considerable, that the aqueous humour often spirts out with great force when the operation is performed. On the other hand, Mr. Schweigger, one of Von Graefe's pupils, holds that severing of the anterior tendinous insertions of the tensor choroideæ is most instrumental in effecting the cure, while the surgeons of Bonn are of opinion that the effect is to be compared to that of an ample incision into any inflamed tissue, as for instance in panaritium, abscess, &c., when the inflammation generally subsides immediately afterwards. Iridectomy is also advisable as a preparatory operation for linear excision of cataract, as sometimes after the latter has been performed, a considerable tumescence of the fragments of the lens, and pressure on, and irritation of, the iris take place. The operation, which causes only a very slight wound, is scarcely ever followed by bad symptoms, so that the patients are generally able to leave their beds a few days afterwards. The enlargement of the pupil by the coloboma iridis and the disfigurement of the eye, upon which the opponents to this operation have laid so much stress, are scarcely perceptible if the incision is made superiorly and internally.

With regard to the operations for cataract, I may mention that German oculists have now almost entirely abandoned reclination, as they found that even in successful cases, where the patients could see very well for some time after the operation had been performed, after a few years, blindness recurred; and in such cases the examination by means of the ophthalmoscope always showed that, where the lens had been placed, a choroiditis had set in, which again destroyed the sight. The results which are obtained by the ordinary extraction by a flap formed upwards for the hard cataract of old people, and by the linear incision for the soft cataract of young persons, are so satisfactory that

other modes of operation are scarcely necessary. In thirty-five cases of extraction by flap, Professor Weber has only had two unfavourable results; and even these patients completely recovered the sight of the other eye. As to linear incision, the same surgeon strongly recommends the operation to be finished as rapidly as possible, and to avoid bruising the edges of the wound by too diligently endeavouring to remove every small fragment of the cataract, as by such proceeding iritis may be induced.—*Med. Times and Gazette, Feb.* 9, 1861, *p.* 155.

78.—ON A NEW OPERATION FOR IRIDECTOMY.

By Henry Greenway, Esq., Plymouth.

[The operation proposed by Mr. Greenway is entirely novel. It is for producing an artificial pupil in the natural position and of a circular form. The author states that the operation is not suitable for all cases.]

Fig. 1.

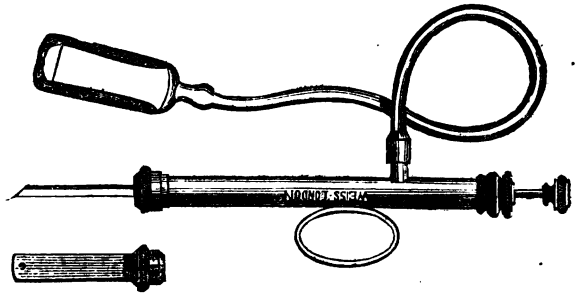


Fig. 2.

Fig. 1.—The instrument complete, showing a side view of the canula.
Fig. 2.—A view of the plane surface of the canula, showing the circular aperture for the admission of iris.

The instrument somewhat resembles a syringe, but, instead of being furnished with an ordinary nozzle, a canula is screwed on to the end of the cylinder, or body, and, there being no piston, the rod, which would otherwise be the piston-rod, is continuous with a blade which lies within the canula. This rod passes through an air-tight fitting at the upper part of the cylinder, and has a spring action. The canula is plane-convex transversely, its bore being one-twelfth of an inch by one-fifth; the plane surface presents near the extremity a circular aperture, about one-eighth of an inch in diameter, for the admission of a portion of the iris; that small portion of the tube beyond this aperture is filled with lead, which serves, not only as a plug to completely obstruct the extremity, but as a point of resistance for the blade. On

the convex surface is a small mark which indicates the position of the aperture on the plane surface.

The blade is kept in close contact with the floor or plane surface of the canula by means of a spring, which is interposed between it and the roof or concavity of the canula. Communicating with the upper part of the cylinder is a metal tube, about an inch in length, on which is fixed an India-rubber tube, which is furnished with a mouth-piece at its free end. This may be termed the suction-tube. On the opposite side of the cylinder is fixed a ring to receive the finger of the operator.

It will be evident that if a person placed the mouth-piece between his lips, and caused suction, a current of air would pass in at the circular aperture of the canula, and, that if this aperture were placed in apposition with a thin structure, such as the iris, a small portion of it would be drawn into the canula in the form of a cup. If, now, the blade be pushed forwards by pressing the head of the rod, the enclosed structure, answering to the shape of the aperture, would be excised, immediately the edge of the blade came in contact with the piece of lead which fills the extremity of the canula beyond the verge of the aperture.

The experiments I have made on the eyes of animals, supplied from the slaughter-house, have been most successful. I found that I could, without fail, excise a perfect disc from the iris in its natural state, thereby leaving a corresponding aperture. In some cases, I produced an aperture so near the pupillary margin as to leave only one-twentieth of an inch of iris, and that in an uninjured state, between the natural pupil and the artificial one. I may, therefore, fairly presume that in cases of closed pupil, for which the instrument is intended, the results will be equally satisfactory.

The mode of operating is to pass a lancet-shaped blade through the temporal edge of the cornea, very near its junction with the sclerotica, into the anterior chamber, the incision being made large enough to admit the canula,—the division of one-fifth of the circumference of the cornea would certainly suffice. The operator having placed the mouth-piece of the suction-tube between his lips, should then introduce the canula into the anterior chamber, its plane surface, of course, facing backwards, and, as soon as the aperture of the canula is over the portion of iris required to be excised, suction should be made, and, if the operator is satisfied that the iris is sufficiently drawn in, pressure should be made on the head of the rod, to cause the edge of the blade to bear on the lead plug to ensure complete excision of the iris. Before the canula is withdrawn from the eye, the blade should be allowed to recede, by ceasing to press on the rod.

The instrument should be held by placing it between the fore and middle fingers of one hand, the thumb resting on the head of the rod; or, between the thumb and middle finger, the fore-finger being then placed on the head of the rod. The other hand should rest on the

patient's cheek, the canula being steadied between the thumb and fore-finger.

The position of the patient during the operation should be upright, the head slightly inclined forwards, so that the iris may have a tendency to gravitate against the instrument. Proper means, of course, would be used to steady the head.

After every operation, the canula should be removed from the cylinder, and thoroughly cleaned and dried; the knife, also, should be cleaned and slightly oiled before it is replaced in the canula.

It would be advisable, before operating on the living subject, to procure a bullock's eye, in order to judge of the amount of suction required.

The chief novelty in this Guillotine is, the employment of a vacuum to perform the office of a forceps, and, in combination with the aperture, to determine the shape of the excision. I believe, in all cases where exhaustion of air has been employed in surgery, it has been simply to do the duty of a pump in raising fluid to the surface. Although this instrument was devised for producing artificial pupil, I intend applying it to other purposes, by merely altering the size and shape of the canula and blade according to the requirements of the case. The operation for the removal of the tonsils is one in which this instrument may be used with advantage, as no injury to the surrounding parts could possibly occur. Having ascertained that the aperture of the canula is closed with the surface of the enlarged tonsil, suction should be made until a sufficient amount of its substance was drawn in. Excision can immediately be produced, even if the patient's mouth were nearly closed. If the operator choose, he may, instead of using the mouth-piece, remove it from the suction-tube, and in its place apply an exhausting syringe, such as is used on a cupping-glass. This may be worked by any by stander under the direction of the surgeon.

In all operations the suction should be maintained during the action of the blade.

In this paper I have not referred to any of the complications which occur in connexion with closure of the pupil; but I do not consider either form of synechia would be any impediment to the use of the instrument.

Messrs. Weiss are the makers of the Guillotine, their name being a sufficient guarantee for its quality and finish.—*Med. Times and Gazette, Dec.* 15, 1860, *p.* 578.

79.—*On Iridectomy in Glaucoma.* By Dr. QUAGLINO.—The following are Dr. Quaglino's conclusions with respect to this operation, the results of abundant clinical observation: 1. Iridectomy may restore or improve vision even in cases in which the glaucoma is chronic, and more or less complete amaurosis has existed for a longer

or shorter time, and signs of congestion of the external membranes are absent. 2. It is indicated in chronic glaucoma, when ophthalmoscopic inspection shows that the papilla and its vessels are not in an advanced state of atrophy. The failures of the operation in old glaucoma have depended upon this advanced condition of atrophy of the anterior extremity of the optic nerve and its central vessels. 3. In the majority of cases the neuralgia which accompanies the glaucomatous affection disappears after iridectomy. 4. The phenomena of excavation of the papilla, its greenish colouration, and pulsatile action of the vessels, indicated by authors as characteristic of glaucoma, may be wanting, while those derived from the exiguity of the arteries and veins of the retina, especially in the papillary area, and the shortness of their course, are more constant. 5. This change in the papillary vessels, which in old glaucoma terminates in atrophy, must be regarded as the effect of the pressure of the humours which have become increased in quantity, or as the consequence of the strangulation produced around the optic nerve by the engorged or hypertrophied choroid. 6. Iridectomy, therefore, cures glaucoma by evacuating the humours, taking off compression by diminishing the mass of tissues within the eyeball, and disgorging the choroidal vessels. A proof that this is the case is found in the fact that after the operation the nutrition of the vessels becomes augmented, and the hardness characteristic of the glaucomatous eye ceases. 7. The success of iridectomy, independently of other circumstances, is directly proportionate to the amount of inflammatory action which succeeds to the operation, and to the size of the portion of the iris removed. 8. After the operation, the anterior chamber being for the most part abolished, and the convexity of the ball diminished, sensible advantage is usually derived from the employment of presbyopic glasses of an intermediate number.—*Omodei's Annali.—Med. Times and Gazette, Feb.* 2, 1861, *p.* 129.

80.—ON THE TREATMENT OF ACUTE GLAUCOMA BY IRIDECTOMY.

[The following case occurred at the Moorfields Ophthalmic Hospital, and was under the care of Mr. Dixon.]

"Mrs. P., aged 46, was admitted December 27th. She was a thin, moderately florid woman, not subject to gout or rheumatism. Twelve days before admission, she had severe pain in the right eye, which came on suddenly at night. Next morning, she found that she could scarcely see with it; and the pain, still severe, had continued up to the time when Mr. Dixon saw her. Blisters and various applications had been used, but without any benefit. When admitted, she could with the affected eye only just distinguish light from darkness, and could not count fingers held up between the eye and the light. The sclerotic was dusky, leadened-coloured, and crossed by enlarged veins.

The pupil was dilated moderately, and fixed. The fundus, as seen through the pupil, looked dull and yellow. The cornea had lost its brilliancy. Atropine did not produce any effect upon the size of the pupil; whilst in the other eye the pupil readily dilated under its use. The ophthalmoscope was used; but a mere red reflex from the fundus was seen. The woman would not submit to the operation of iridectomy, which was at once proposed, until the 31st, the fourteenth day from the attack, when iridectomy was performed. The piece of iris removed was from the upper part. No chloroform was given. The pain was at once relieved.

"Four days after the operation, she had had no return of pain, and could read No. 10 of Jäger's test-types. There was a minute fistula in the cornea, to which Mr. Dixon applied nitrate of silver. She gradually improved, and was soon able to read small type.

"On Jan. 25th, the left eye became suddenly very painful. The pain was in the globe; it was very intense, and was accompanied by vomiting. She was not seen by Mr. Dixon until the 28th. He then found that the eye was in exactly the same state as the right had been. She could only just count fingers. The pupil was dilated and fixed; and the fundus could not be illuminated by the ophthalmoscope. Iridectomy was at once performed, and with the same amount of success as attended the operation on the other eye. She can now see well with both eyes, and can read pearl type without the aid of glasses.

"Mr. Dixon remarked that, in this case, the disease had been of the exact form to the relief of which iridectomy is best suited; viz., *acute* glaucoma. In the chronic cases, as far as he had seen, little or no benefit followed the operation, but rather an increase of the irritation. It is interesting to note that the attack in the left eye occurred whilst the right eye was going on satisfactorily. The ophthalmoscope had not been used to examine the sound eye, and therefore no blame was traceable to it, as having re-excited the disease in it. The occurrence of glaucoma in the second eye, a few weeks after it has attacked the first is quite in accordance with what is frequently observed; and hence, as both eyes generally suffer, the value of iridectomy is much increased."

[We append the remarks of the Editor of the British Medical Journal on glaucoma and iridectomy, published in the same number of that Journal, and particularly referring to the above case.]

The operation of iridectomy in acute glaucoma has now been before the profession for about six years. It has been largely practised; and, by those who have avowedly abstained from trying it, energetic attempts have been made to detract from its merits. The great facts, however, remain, that acute glaucoma does not unfrequently occur; that it is irremediable by constitutional treatment; and that the excision of a portion of iris, if performed within a few days of the out-

break, is effectual in restoring vision and putting an end to the disease. To the first two of these propositions all experience bears testimony. Respecting the third, when surgeons of the authority which Mr. Bowman, Mr. Dixon, Mr. Critchett, and Mr. Hulke, must be allowed to possess in such a matter, unite in one strong and unequivocal statement, it is no longer possible to be incredulous. Indeed, the facts are before us, and we may now judge for ourselves. Cases attested by the names of the surgeons we have mentioned are now on record, which, if clinical evidence can be allowed the slightest weight, prove that this operation, whatever may be the true theory of its action, possesses a really marvellous power in controlling the progress of certain morbid changes otherwise destined to result in permanent blindness. If we may trust these facts, Von Gräfe's discovery ranks second only in ophthalmic beneficence to that of the operation for cataract.

Let us ask especial attention to a case recently under Mr. Dixon's care at the Moorfields Hospital (see above). Mr. Dixon's reputation as a sound and most cautious surgeon is well known. He was not one of those who early adopted this new operation; he even published certain warnings against its too indiscriminate performance. Being, therefore, no partisan, his facts possess especial value. In the case we refer to, the disease was well characterised, and had existed for nearly a fortnight. The woman was so far blind with the affected eye that she could only see a hand moving before it, and could not count fingers. The surface of her cornea was so uneven that no images were reflected with clearness; and such was the effect of the intraocular pressure, that it was impossible to light up the fundus with the ophthalmoscope. Thus the sign of cupping of the optic entrance could not be used, the case being too advanced in its stage. In this condition of things, Mr. Dixon excised a portion of iris. All pain in the globe ceased after the operation. On the next day, the woman could easily see ordinary objects. On the third, the brilliancy of her cornea was restored, and she could read large print. At a week's end, she could read common type with an eye with which previously she could not discern fingers. Subsequently the same disease attacked the other eye. Iridectomy was performed in it also, and with an equally good result.

The case, wonderful as it is, coincides so exactly with fifty others published by different observers, that there is no room for fallacy. It is no longer a matter in which the conscientious surgeon can decline to act. If he operates, his patients will regain their sight; if not, they will go blind. To dissuade a patient with well characterised glaucoma, *in an acute stage*, from having a portion of iris excised, will, if we mistake not, soon become as legitimate a ground for an action for malpraxis as to leave a dislocated hip unreduced.

It must be remembered—and it is for this reason that we wish especially to press the subject upon the attention of all our readers—

that acute glaucoma is a condition which will not wait. If the proper time is not seized, it passes by for ever. In this respect, it is more urgent than any form of dislocation, and in fact closely resembles strangulated hernia. There are a few operations which, being usually required at an emergency, it is considered that every surgeon ought to be able to perform at a moment's notice. Amongst these, tracheotomy and the operation for hernia have hitherto been the chief. Operations on the eye have been regarded as a speciality, and as requiring special practice for their successful performance. In iridectomy we have, however, an exception to this. Von Gräfe, in his third memoir on this subject, has very properly insisted that the surgeon in a remote country district ought to feel himself as much bound to be able to perform iridectomy when requisite, as he would to practise tracheotomy off-hand if a patient were in danger of laryngeal occlusion. The operation is in itself not a difficult one, and the delay of a few days may be of the utmost consequence.

There are some interesting facts in the history of the progress of this new operation into general adoption. It suffered temporarily, as many other important novelties have done, through the honesty of its advocates. Having discovered its extreme value in acute cases, Mr. Bowman and Mr. Critchett, who were the first to employ it in this country, felt bound also to give to numerous other cases, otherwise hopeless, the chance which it might possibly afford. It seemed cruel to refuse to do it because only a half-success, or perhaps still less, was all that the most sanguine could hope for. Hence its performance in many cases which from the first were almost hopeless. All these were scrupulously recorded by Dr. Bader in the Ophthalmic Hospital Reports, and supplied materials for attacks upon the operation itself from the indiscriminating and prejudiced. Just so did the operation of trephining in cases of cranial fracture with depression fall for a time into great disfavour. It was performed in all severe cases, and of course a large majority of the patients died, much as they would have died had they been let alone. All the cases were lumped together, and it then appeared that not one in ten of our trephined cases resulted in recovery; *ergo*, trephining was a bad operation, and to be disused. After not a few lives have been sacrificed to this miserable logic, the trephine is now regaining the legitimate confidence of surgeons. Had our iridectomists selected their cases for operation, or had they published only their very successful ones (*i.e.* those done in the acute stage), their triumph would have been much sooner and more loudly hailed. We need not remark, however, that the true interests of science would not have been served nearly so efficiently as they have been by the manly course which was pursued.—*Brit. Med. Journal, March* 16, 1861, *pp.* 272, 280.

81.—ON IRIDECTOMY IN GLAUCOMA, WITH A DESCRIPTION OF A NEW METHOD OF OPERATING.

By Thomas Nunneley, Esq., Leeds.

[The following paper by Mr. Nunneley it will be observed recommends nearly the same operative procedure as that by Mr. Hancock, recorded in our last volume, (p. 270), although some changes are made in the operation, and the name is changed. We are sorry that Mr. Hancock's valuable paper is scarcely referred to.

Few ophthalmic surgeons really believe that the true position of the operation of iridectomy, as devised by Von Graefe, has yet been determined. Glaucoma depends upon inflammation or congestion of the choroid in the first place, the pressure is secondary. Von Graefe's operation, Mr. N. thinks, is only calculated to remove the effect of the disease; it has no power whatever over the original cause of it, the inflammation.]

Surely, in contending for the correctness of the supposition of the choroid being primarily complicated in glaucoma, which indeed appears most probable in at least many cases, our attention should be earnestly directed to the causes which first induced this condition. As it existed prior to the intra-ocular pressure, which it produces, whatever mischief this causes, or however relieved, it will in all probability continue to exist, even supposing the pressure to be removed, unless means be also found for its cure. This, however, apparently self-evident proposition need not, according to Von Graefe, be regarded. Only remove the pressure, and the eye, if not already absolutely disorganized, is perfectly cured by iridectomy alone. This doctrine appears to be so broadly stated by Von Graefe in the memoirs referred to as to be likely, if generally received, as from the great authority of the writer there is fear of, to be productive of great mischief by leading to a neglect of careful treatment in the earlier stages of these affections, when alone it can be really useful, and of encouraging a reliance upon a reckless resort to a serious operation as all that is requisite in the whole class of these affections, and that at a time when no benefit can possibly ensue, but, as results have proved, where danger of serious mischief supervening is not improbable.

But while endeavouring to recall attention to the necessity of carefully treating every early symptom, both local and general, and attempting by every curative means to prevent intra-ocular pressure by removing that condition of the choroid which gives rise to it, and, even after operation, of still striving to remove the primary cause of the mischief, we must feel grateful to Von Graefe for improving upon the efficacy of former modes of paracentesis by a needle or knife, and showing the greater success of another operation; for though we may not agree with him that iridectomy is the only thing requisite, that it is always free from ill consequences, and always to be practised, there can be no doubt that the larger operation has been of the greatest

benefit in preserving vision in many eyes so affected. With these reservations, I do not think the argument which has been advanced against iridectomy—that it is merely a relief of a secondary affection, even according to Von Graefe's own confession—is of any great value; for in how many diseases is not interference directed to the removal of secondary affections, which of themselves may continue after it has subsided, and may be more serious and more permanent than the original disease which induced them? Inflammation of the urethra produces stricture; inflammation of the lachrymal sac, fistula; pressure upon an artery, adhesion of its walls; inflammation of the serous membranes, various effusions into the large cavities; of the mucous membranes, ulcerations; of the synovial membranes, disease of cartilage, and so on. Hence, while we do not neglect attention to the primary affection, the removal of the secondary is most important. That, in many cases, improvement in vision follows iridectomy is, I think, certain. How, then, is this effected? In iridectomy, is the removal of a large portion of the iris the essential part of the operation? Is the operation successful in spite of the mischie finflicted by the removal of a large piece of the iris, owing to something else which is done during this removal? And, if the former, why or how does this occur? if the latter, in what does this consist? Graefe, and those who are convinced by his reasoning, and think his facts incontrovertible, unhesitatingly declare the removal of the iris to be the essence of the operation, without which, according to them, it would be a failure, and as useless as they say paracentesis is. They attribute permanent success to a diminution in the extent of iris surface, so that thereafter there is a lessened power of secreting aqueous humour, by which the tension of the whole globe is removed, and the optic papilla preserved in its integrity, or recovers this in case pressure has not already too far disorganized it; at the same time the cornea, which had been rendered flatter, recovers its normal curve, and the vitreous humour becomes softer and clearer.

To some, at least, of these suppositions I would venture to demur. I would remark in the first place, that the idea of there being less secretion of vitreous humour by the removal of the inflammation and congestion of the choroid through the evacuation of the aqueous humour and ablation of a portion of the iris, is an assumption and not an ascertained fact; and, secondly, even if this be so, the notion that removing a part of the iris proportionally lessens the secretive power of aqueous humour is a mere hypothesis resting upon a very problematical supposition. That the iris is the source whence the aqueous humour is derived is by no means proved, and, to say the least, is very doubtful. I know of no anatomical fact which would prove the iris to be the secretor of this humour; for while all microscopists agree in the presence of epithelial cells upon the posterior surface of the cornea, there is no such agreement of opinion as to their existence upon the iris. While Hassall, Kölliker, and some others, believe that these cells

are continued upon the anterior surface of the iris, Bowman decidedly denies it: and though I have in one or two instances seen them, in the great majority of examinations I have altogether failed in finding them. So, then, this surface is certainly not more adapted for the office than is the cornea; and it will surely not be contended by any one that the posterior surface of the iris is more so than the ciliary processes. Hence we can hardly admit as of much value the explanation put forward as to the *modus operandi* of iridectomy.

The necessity for the removal of a portion of the iris, and the statement that the larger the piece removed the greater is the certainty of success, is a question of fact to be determined by observation alone. Upon the question of fact I incline to join issue, and think that the removal of the iris *per se* is an evil to be avoided if possible—one which in recent cases may commonly, and in those of longer standing not unfrequently, be done. The good accompanying its removal does not, in my judgment, result from the loss of the iris itself, but from allowing a greater yielding of the eye-ball—in all probability owing to a greater division of its curve being made when a large portion of the iris is taken away than when none of it is removed, and thus permanently lessening its tension, as well as affording a longer continued drain of the aqueous humour.

Though it has been stated by Von. Graefe that the eye becomes presbyopic in glaucoma, owing to the intra-ocular pressure causing a flattening of the normal curvature of the cornea, which iridectomy removes at the same time and by the same means as it does the hollowing of the optic papilla, this change of curvature, as an invariable symptom of glaucoma, and as a necessary consequence of intra-ocular pressure, has been denied by other good observers. So far as my observation extends, it is by no means constant. Indeed, I think I have seen great intra-ocular pressure, with an increase in the curve of the cornea, in some cases where the cornea has been more yielding than the sclerotic. That the cornea frequently is flattened is certain, and that it then becomes more prominent after operation is also true; not so much, however, I apprehend, from lessened tension within as from diminished resistance in itself. The division of so much of its connexion with the sclerotic (and these tissues are really one, so that whether the incision be made a little more forward or backward is of small importance) has weakened its power. By the operation I propose as a substitute, this increase in the corneal curvature is certainly obtained in a marked degree, more so, I think, than in Von Graefe's iridectomy.

I believe, then, all that iridectomy accomplishes in the cure of acute glaucoma and glaucomatous diseases is in the greater degree and more permanent manner in which it affords relief to intra-ocular pressure than paracentesis, as performed previous to its introduction, did.

Can, then, any simpler means be devised for obtaining this desired result than removing a large portion of the iris? I think it can.

Observing that the eyeball is often distended to the utmost limit which the comparatively unyielding sclerotic and cornea will allow, and that the pain and acutely distressing symptoms in the ball and about the orbit commonly occur in proportion to the rapidity with whichthe distension takes place, whether the disease be glaucoma, iritis, or choroido-iritis,—and knowing that the most unyielding portion of the globe is the point of junction of the sclerotic, cornea, iris, and ciliary muscle, which may not unfrequently, in very decided cases of hydro-ophthalmia, be observed as a depressed ring between the bulging sclerotic and cornea,—it occurred to me that division of this part would afford the desired relief, and that not improbably the good gained in Von Graefe's operation in reality depends upon the removal of the resistance of this part, and not upon the ablation of the iris. This result, I think, has been proved. I have waited before announcing it until I have had a sufficient number of cases as fairly to admit of a comparative estimate of it with iridectomy. I have now operated upon about fifteen eyes by the method of Von Graefe, and on certainly not less than twenty eyes in the manner I propose to describe. The result has been in favour of the latter operation. It has, so far as I can judge, afforded all the relief that the more serious proceeding has done, and appears to be free from its inconveniences. It is easier performed, produces much less deformity, inflicts much less mischief upon the eye, is followed by as much diminution of the intra-ocular tension, by as great or even greater increase in the corneal curvature, and the relief has been quite as permanent. The space over which my observation has extended is upwards of eighteen months.

The manner in which I have operated is to puncture the sclerotic coat with the point of a sharp, thin knife—a small cataract or very narrow, short bistoury answers very well—not less than one-eighth of an inch behind its junction with the cornea, and carry it on to about the same extent through the cornea, making altogether an incision about one third of an inch long. Care must be taken to pass the knife sufficiently deep to completely divide these textures, and yet not so deep as to touch the lens, which I once did, owing to the patient starting at the moment the incision was made. Care also must be taken not to make the incision too long. A larger incision in the sclerotic, besides unnecessarily wounding important tissues, is useless, and if carried too far towards the centre of the cornea, though allowing this afterwards to yield more, is bad, for it may allow the lens to be displaced into the aqueous chamber; and if the iris should adhere to the whole extent of the corneal section, as it is likely to do, particularly if a portion of its whole breadth has been removed, not only will there be dragging of it, but the section becomes opaque, and hence the field of vision is lessened. In making the section, if the point of the knife has been well kept in, the outer margin of the iris will be divided. Sometimes the iris bulges through the section. I have tried the effect of simply leaving the prolapsed iris in the wound, of cutting it off, and also of

pulling out a larger portion, and cutting off a strip through the entire width. In this latter plan the operation more nearly assimilates with Von Graefe's iridectomy, only that the section through the unyielding tissues is made directly across their junction, instead of into or parallel with it, whereby a greater expansion in it is allowed, and not nearly so much of the iris is removed. If none of the iris be cut off or tied, the pupil usually recovers its circular form; if some be excised, it remains oval and attached to the corneal cicatrix in proportion to the size of the piece removed, but in a much less degree than would *à priori* be anticipated. The degree of deformity is very slight indeed.

The spot where the incision is made, so far as the relief obtained is concerned, can make very little difference; but I have usually selected the centre of the lower corneal curvature as being likely to produce less noticeable alteration in the appearance of the eye; and, when both eyes are operated upon, as interfering less with accuracy of vision than any other. On the whole I think, in severe cases, the removal of some small section of the entire width of the iris advantageous, as affording a longer continuous drain of the aqueous humour than simple incision of the scleroto-corneal junction does. I have in one case only seen much bleeding into the aqueous chamber, and what blood is there effused is soon absorbed. In one case the iris gave out more blood than usual—perhaps twenty drops; but as this at once escaped by the section, it was of no moment, and was easily arrested by cold, wet cloths. In two of the cases in which I performed iridectomy, the quantity of blood effused into the eye was sufficient to fill the aqueous chamber; and in one of them it was a long time before being entirely absorbed. Usually, however, all trace of it is lost within a week, or, at most, ten days. In two or three of the cases severe pain in the side of the head ensued, which an opiate relieved; and in one instance, as I have before said, considerable inflammation; but in all other cases very little inconvenience followed.

In hardly one instance have these operations been performed in really strong, vigorous persons. The majority were females. Though two were large and apparently strong, vigorous young women, both had been in bad health for several months. In all, after the immediate effects of the operation and the more acute symptoms have subsided, I have thought it proper to continue for many weeks, or even months, constitutional treatment. Principally iron, iodine, hydriodate of potassa, with strychnia or quinine, and mercurial purgatives, have been given, by which the constitution has been materially invigorated, and I have no doubt the eyes, in a corresponding degree, rendered more healthy.

I have also performed this operation upon both eyes in an extremely bad case of conical cornea, where the alteration in the curve of the cornea had been progressing for nearly ten years, until the sight was so bad that the patient had been unable to do even the weaving of

plain, coarse woollen cloth, where hardly sight is required, with the happiest result, as the man is now not merely enabled to work at a better description of goods, and thus support himself and family, but possesses a very fair range of vision; indeed this operation, with a little modification, appears remarkably well adapted for the relief of this complaint. The section of the scleroto-corneal junction allows the periphery of the cornea to bulge out, and thus the convexity of the whole surface of the cornea is lessened: and by drawing out not too large a portion of the whole width of the iris, and either cutting it off, or, still better, fixing it at the wound by a fine ligature, adhesion takes place, and a good contractile pupil is made near to the margin of the cornea. The best place for the section to be made in these cases is at the outer and lower margin of the cornea, by which a large field of vision is obtained.

Mr. Hancock has recently written more than one paper in the Lancet on glaucoma and the relief of it by operation, in which he controverts both the theory and the practice of Von Graefe, maintaining both are wrong. Mr. Hancock believes the disease does not depend upon hypersecretion of the vitreous humour owing to disease of the choroid coat, but that it essentially depends upon continuous spasm of the ciliary muscle, which, according to him, induces the hardness in the globe of the eye. Hence the proper treatment, he maintains, is not ablation of a portion of the iris, but division of the ciliary muscle by a small incision through the sclerotic coat immediately over it, and parallel with its fibres, by which he considers the spasm is overcome. He has published cases in which he and other surgeons have operated with success, notwithstanding, as he says, Mr. Hulke has denied the possible success of any other method than Von Graefe's iridectomy. Mr. Hulke, who is strongly impressed with the superior curative power of iridectomy, has, to prove the impossibility of Mr. Hancock's theory being true, referred to the dissection of glaucomatous eyes, in which he states that the ciliary muscle has been found atrophied and completely shrunken—a condition which Mr. Hulke considers incompatible with hyperaction. On the other hand, in rejoinder, Mr. Hancock informs us that his colleague, Mr. Hogg, has dissected glaucomatous eyes, in which the ciliary muscle has been found to be decidedly hypertrophied, and therefore must, they think, have produced the disease in the way he contends for.

I would venture to remark that I think very little value is to be attached to any of these dissections, because, in the first set (those of Mr. Hulke), if, as is almost certain was the case, the glaucomatous condition was of old standing, it is hardly probable the ciliary muscle alone was atrophied, but that its wasted state formed part of a general ocular condition, and as such, proves nothing as to the ciliary muscle alone; while as to the second set, those of Mr. Hogg, we all know that a slight variation in the size and thickness of the ciliary muscle may and does occur in different eyes, without there being any suspicion of

diseased hypertrophy. Besides it must be most difficult to detect any such minute change, as mere simple hypertrophy in so small a muscle, if confined to it alone, would occasion; and in an eye really glaucomatous it is most improbable this small ciliary ring would alone be found altered whatever the alteration may be, and hence, like Mr. Hulke's cases, of no value as to the true condition of the ciliary muscle alone. The dissection of a sufficient number of glaucomatous eyes, under circumstances to be of much value, is hardly likely to happen to anyone, as happily few persons die, at any rate with the affection in an early stage, and hence the opportunities of seeing the disease uncomplicated are very rare.

It cannot, however, I think, be at all difficult to show that neither the theory nor the practice, as applicable to the theory of Mr. Hancock, can possibly be true, without in any degree disputing his statement of improvement following his operation, which appears most probable.

Supposing the ciliary muscle were really hypertrophied and in a state of tonic spasm, how could it induce hardness of the whole globe of the eye, flattening of the cornea, and much intraocular pressure? Muscles unstriped as well as striped act only in proportion to their length, in the direction of their fibres, and towards their most fixed attachment. The fibres of the ciliary muscle, firmly attached to the scleroto-corneal junction, radiate outwards and backwards from this point upon the soft yielding choroid. The fibres are not circular, but straight, and not more than one-seventh of an inch long. They can have no action whatever upon the sclerotic coat, to which they are not attached posteriorly; they therefore cannot render it hard. Whatever effect they may have upon the choroid would rather be to pull it away from the sclerotic, and hence, if of any effect upon the latter, to render it less tense. Though the ciliary muscle be called by some the tensor of the choroid, it can hardly act much in this manner upon this coat, whatever it may upon the ciliary processes. But even were this tensile action proved, and the muscle contracted to its entire length, the choroid coat is too extensive, too lax, and too yielding to allow of the muscle dragging the whole coat inwards with sufficient force to compress the retina and vitreous humour in any great degree, and it certainly could by no possibility cause any atrophy or cup-shaped depression of the optic-nerve papilla, as all agree does occur in glaucoma; since the cribriform part of the sclerotic coat is *outside* the choroid, and therefore altogether beyond even the theoretical power of the ciliary muscle.

But even were this theory of compression by the ciliary muscle as true as it appears untenable, how could a simple puncture in the direction of its fibres interfere with the action of the entire circle of its fibres? A broad transverse incision in the direction of the corneal curve, by dividing the fibres, might act powerfully in proportion to the number of fibres divided, but that a simple momentary separation from each other of two or three adjoining parallel fibres, without any

division of their structure—for such must be the effect of a fine, sharp, thin knife,—could permanently arrest strong spasmodic action in an entire muscle spread over a large circle, it is impossible to conceive.

Mr. Hancock's practice I imagine to be more successful than his theory. It will, I apprehend, be found successful in proportion as the most unyielding part of the outer tissues of the globe are divided; and as there can be no doubt of the correctness of the observed facts of improvement in vision following Mr. Hancock's operation, I think I may claim his cases as strongly in favour of the operation I have suggested, and in support of the explanation I have ventured to give as to the *rationale* of it,—namely, that the improvement following his operation results from a division of the unyielding outer coats of the eyeball, and not, as he supposes, by overcoming spasm in the ciliary muscle, or, as Mr. Hulke thinks, merely by paracentesis, as a simple puncture in any other part of the eye would do, and which has in other hands so uniformly failed in affording more than the most temporary relief.—*Lancet, Jan.* 19 *and* 26, 1861, *pp.* 55, 82.

82.—ON THE FORMATION OF ARTIFICIAL PUPIL BY IRIDDESIS.

By GEORGE CRITCHETT, Esq., Surgeon to the Royal London Ophthalmic Hospital.

In a former paper (see *Retrospect*, vol. xxxvii, p. 439, vol. xxxviii, p. 245,) I have described a method of forming an artificial pupil, then recently adopted in several instances, and that I had reason to believe possessed some important advantages over the operations usually had recourse to. It consisted in drawing a portion of the iris through a small wound in the cornea, and tying it with a fine loop of silk; its advantages, as I mentioned in that paper, are, that the iris-fibres are not torn or divided, that the pupil is merely altered in its shape and position, and retains permanently a definite size, a clearly defined boundary and contractility under the influence of light. Again, by this method we can regulate with great exactness the size and position of the pupil, and thus obtain every optical advantage that the circumstances and specialities of each individual case permit.

My present object is to describe some modifications that I have found it convenient to make in performing the operation of iriddesis, and also to describe some cases illustrative of the various morbid conditions for which this operation is suited.

In order to perform this operation with the requisite amount of minuteness and precision, a perfectly quiet state on the part of the patient is important. Except in rare cases this is best accomplished by the use of *chloroform*. The eye having been exposed by means of the wire speculum, and fixed in a convenient position, a broad needle must be entered close to the junction of the cornea and sclerotic,

and an opening made *just of sufficient size* to admit the canula forceps. A small loop of fine *floss* silk, moistened, must then be *laid on the eye over the opening in the cornea.* The canula forceps is then introduced through the loop and the opening in the cornea into the anterior chamber. The iris is then seized about midway between the ciliary and pupillary margin, or sometimes, if possible, rather nearer to the ciliary margin, and drawn out *to the requisite extent.* An assistant seizes the two ends of the loop with small broad-ended forceps, and *carefully ties it upon the eye,* so as to include and strangulate the portion of iris that has been drawn out. The ends are then cut off, and the operation is completed.

Usually about the second day all trace of the ligatured portion of the iris is gone, and the bit of silk, if it has not been washed off, may be taken away. It occasionally happens that the ligatured portion of iris, with the piece of silk attached to it, is drawn somewhat within the wound; when this is the case an end of the silk must be seized with forceps and drawn away. The possibility of such an occurrence proves the necessity of leaving the *ends of the knot rather long.* It is very rarely indeed that any irritation of consequence follows this operation; the iris becomes permanently adherent at the spot where it has been tied, so as to fix the pupil in its altered form and position.

This method of operating has been adopted with favourable results in the following groups of cases:—

1st. *Central leucoma,* corresponding in size and position to the natural pupil.

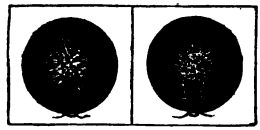
2nd. Where there has been a *penetrating ulcer* of the cornea, with some surrounding opacity, and the pupil is altered in shape, diminished in size, and drawn towards or behind the opacity.

3rd. Cases of *opaque capsule, with adherent pupil,* in which it is desirable to enlarge the pupil slightly in one direction, so as to expose a clear part of the lens.

I have performed the operation in each of these groups of cases on several occasions, and I will select a specimen of each, as it may serve further to illustrate and explain the principles of the operation.

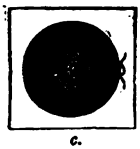
1. In November 1858, a boy, aged 14, was brought to me by his mother at the Royal London Ophthalmic Hospital. He had dense central opacity of the cornea of both eyes, the result of severe strumous ophthalmia about five years previously. He could just manage to guide himself about, but his sight was so imperfect that he was unable to read even large print, and his education in consequence was entirely neglected; the pupils were active, but when contracted were hidden behind the opacity. The largest clear space of cornea was at the lower part of each. I therefore determined to alter the shape of the pupil in that direction, so as to bring it opposite to a clear part of the cornea; this I did in both eyes. The patient was placed under the influence of chloroform, and I drew out and tied a portion of the iris;

but in the left eye I drew out rather more than I did in the right. The result of this was that the pupil in the right eye only extended to a point midway between the ciliary and pupillary margins of the iris (*a*), and retained its natural contractility, whereas in the left eye it extended as far as the ciliary attachment (*b*) and had lost its natural contractility. The boy made a rapid recovery without any unfavourable symptom. On the third day I was able to remove the silk, all trace of the prolapsed and ligatured portion of iris was gone, and the pupils were exactly of the size and shape that I had originally made them. Some weeks afterwards, when the eyes had completely recovered, I tested the sight, and found that in the right eye, where the pupil was small and moveable, it was much more perfect than in the left, where the pupil extended as far as the ciliary margin. With the right he could read ordinary print, but he could not read at all with the left. This may be partly due to the condition of the cornea in the right eye; but I am inclined to attribute it in a great measure to the more central position, to the mobility and limited size of this pupil, and, above all, to the circumstance of its not extending to the extreme margin of the cornea, as the pupil did in the left eye.



a. *b.*

I have performed the operation in several similar cases, sometimes trying how small a portion I could draw out and tie. On the last occasion, the piece that I drew out, when included in the ligature, was only about the size of a small pin's head, and the pupil was very slightly altered in shape (*c*). The sight in this case was very perfect. The condition of the artificial pupil is not, however, always a fair test of the operation. It may be everything that can be desired, both as regards size and position, and yet the sight may be very imperfect. This may arise from an altered state of the cornea, the result of the same inflammatory attack that produced the opacity, and by the perfect smoothness of its convex surface having been changed into a series of little flat surfaces, or the retina and other structures may have been compromised by the original inflammatory attack.



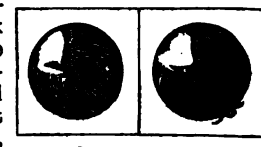
c.

The conditions most favourable for a good result are a *clearly defined dense opacity* in the centre of an otherwise transparent and healthy cornea. If a pupil, rather less than the normal size, and retaining its natural boundary and mobility, be brought opposite to the clear part of the cornea very excellent sight is obtained.

The second group of cases, in which "there has been a penetrating ulcer of the cornea, with some surrounding opacity, and the pupil is altered in shape, diminished in size, and drawn towards or behind the opacity," to which it is adherent at one point, present great variety,

both as regards the size and shape of the remaining pupil, the extent of the opacity, and the condition of the transparent part of the cornea; and upon these circumstances must, in a great measure, depend the degree of sight that is obtained; at the same time, I think it must be admitted that iriddesis is peculiarly well adapted for these cases, enabling us to regulate the size and position of the pupil so as to bring it opposite the best part of the cornea, and at the same time to make it with certainty, small and moveable, and with a well-defined and permanent boundary. I have operated upon several of these cases in this manner with satisfactory results, and will briefly detail one that presented some circumstances of peculiar interest.

2. A gentleman came to consult me, from Australia, suffering from severe ophthalmia in both eyes of about two years' duration; in one, vision was entirely and hopelessly destroyed; in the other, there had been a penetrating ulcer that had produced a dense opacity extending over nearly half the cornea, behind which the pupil, of an oval shape, adherent at one part, and diminished about one-third, was drawn. For some months the eye remained so inflamed and irritable that nothing could be attempted for the improvement of the sight; at last, however, under the influence of a generous diet, change of climate, and soothing topical treatment, the eye regained a healthy condition, the opacity diminished in size, and became more clearly defined and circumscribed, and the pupil extended just to its margin, so that in a subdued light the patient could make out the outline of an object; and under the influence of atropine, a further slight improvement took place, but not to such an extent as to give any useful vision. Under these circumstances, I proceeded to enlarge the pupil downwards and inwards, so as to bring it opposite to the clear part of the cornea, rather less than its normal size, retaining its defined border and its contractility and occupying a position midway between the corneal opacity and the margin of the transparent cornea. The operation was performed in the usual way; two days afterwards the bit of silk came away; every trace of the deligated portion of iris was gone, and in a few days the eye had regained its strength. Before this gentleman left England, he could not only see distant objects well, but could read writing and clear print; and I hear that, since his return to Australia, his sight has still further improved.



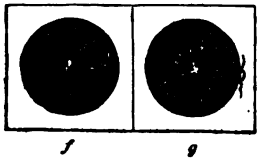
d. *e.*

I think it will be conceded that every possible optical advantage was obtained in this case that the previous condition of the eye permitted, and that any other mode of operating, or any other form of pupil, would have produced less perfect results.

The third group of cases are those in which the pupil is irregular, contracted and opaque, or occupied by an opaque membrane. The object in such cases is to detach the pupil at one point from

its adhesion to the opaque membrane, and then to enlarge it in one direction, so as to discover and expose a clear part of the lens and its capsule. This operation has been performed several times at the Royal London Ophthalmic Hospital, and on some occasions with very satisfactory results. It is more difficult to carry out successfully the various steps of the operation than in either of the other groups. One difficulty is the firmness of the adhesion, and another is the softness and rottenness of the previously diseased iris.

3. In a case that came under my care, I adopted the following method of proceeding. The patient was a female between 40 and 50 years of age, who had formerly suffered from recurrent iritis, which had left a small, irregular, and adherent pupil, and some opacity of the capsule. The eye having been exposed, and fixed with speculum and forceps, and an opening having been made through the cornea, I first introduced Tyrrell's blunt hook, and getting it through the edge of the adherent iris, made gentle traction upon it, so as to detach it from its adhesion. This I succeeded in doing. I then freed the hook from the iris and drew it out. I then introduced my short canula forceps, and seized the iris about midway between its pupillary and ciliary attachment, and drew it out, and tied it in the usual way. The effect of this method of proceeding was to form a small triangular pupil by the side of the opaque part of the capsule, and opposite to a transparent portion of the capsule and lens. The sight was very much improved in this case.

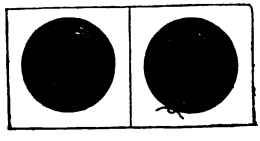


f *g*

This operation has been performed several times at the Royal London Ophthalmic Hospital, both by some of my colleagues, and by myself, and sometimes with a very satisfactory result; it is, however, only applicable to those cases in which the opacity of the capsule is limited to that part around which the contracted margin of the pupil is attached, and in which the rest, both of the capsule and of the lens, retains its transparency. It is scarcely possible, prior to the performance of the operation, to determine this point; but in the event of this further complication being revealed by the artificial pupil, it constitutes a good preliminary for further proceedings, and is in itself very safe. I find by referring to the hospital records, that the operation of iriddesis has been performed by my colleagues and myself sixty-six times during the past two years; and in a large majority of cases with very satisfactory results; I have also adopted it four times in private cases with good success.

It seems highly probable that the range of cases to which the operation is applicable, cannot yet be limited, and in proof of this I have much pleasure in adding a very interesting case furnished me by my friend Mr. Streatfeild, in which the operation has been applied by him in a novel and highly satisfactory manner.

"A farmer, aged about 50 years, came to me at the Moorfields Hospital, on the 15th of September, 1859, he had had cataract of one eye only, for which extraction had been performed about a year previously, with subsequent prolapse of the iris, through the corneal wound, by which not more than, if so much as, one-half of the natural circumference of the pupil was left free (*h*), and this was all above the axis of vision and partly hidden by the upper lid; nevertheless he could, with it, distinguish some objects, and by raising the upper lid and using a (2-biconvex) lens he could read small capital letters.



A. i.

"I performed iriddesis in the downward direction; it was difficult to seize the portion of iris to be tied, as it had lost the support of the lens, its fibres were much stretched, and therefore it receded before the forceps altogether. Having once failed to draw out by the corneal opening, the portion held by the forceps, I succeeded the next time in securing it with the ligature of floss silk.

"On the 19th, (the iris was adherent to the new corneal wound) I found the knot remaining in the cornea deeply, as if drawn into the wound by the tense iris fibres; it had not, however, passed through the cornea, and I readily removed it with forceps.

"The pupil now (*i*) acted perfectly; although before the operation, it had not seemed to act at all. The pupil was now in the axis of vision; but below it is a minute narrow vertical rent in the iris, probably made in the operation, when I, the first time, took hold of a portion of it; this, however, appears to be no great disadvantage, for the man voluntarily, without a lens, said he now could see better than he did.

"On the 26th, the pupil remained as at the last date, and acted well; all that portion of iris not involved in the ill-result of the extraction operation *being now of about its natural width*. With a (2-biconvex) lens, he could already, without effort read, with the eye operated on, rather small type. The eye looks altogether as well as can be, and its external appearance is much improved by the operation on the pupil."

Mr. Streatfeild has since added to the above that he has had "a second case of similar displacement of the pupil after 'extraction.' In this instance the cataract operation had involved the iris to an amount as great as in the first case, but the cornea retained a better form. The patient, however, could not (with a lens) see capitals; but, by iriddesis, he could tell the time by a watch, and, (with a lens) read the smallest type."

I have been anxious, in the present paper, to point out the easiest and best method of performing the operation, to indicate clearly the various classes of cases to which it is applicable, and to give the

results and impressions which a more extended trial has given me. The only objection that I have been able to detect, is an occasional tendency to a drawing in of the loop through the wound into the anterior chamber. If the ends of the silk are left sufficiently long, this may be easily remedied by seizing a loose end and drawing it away three or four days after the operation; but the occurrence itself is exceptional, and may be obviated by making the opening in the cornea very small, and by inclosing the entire piece of prolapsed iris in the silk. The advantages of iriddesis I have already dwelt upon, and may briefly restate:—It is applicable to nearly every variety of case in which an artificial pupil is required; it enables the operator to regulate with accuracy the exact size and position of the pupil he proposes to make; it ensures to it a defined boundary and the natural mobility (provided some portion of the pupil in its natural state existed prior to the performance of the operation); it secures stability to the artificial pupil, so that it will continue of the size and form it had when first made. After a little practice the operation is performed with great ease and certainty, and recovery is almost uniformly rapid and complete. I believe that it possesses important advantages over every method hitherto proposed, and that it enables the operator to ensure every optical advantage that the nature of the cases, and the previous condition of the eyes permit; and I confidently anticipate its very general adoption.—*Ophth. Hospital Reports, Oct.* 1859, *p.* 145.

83.—PRACTICAL OBSERVATIONS UPON CONGENITAL CATARACT.

By GEORGE CRITCHETT, Esq., Surgeon to the Royal London Ophthalmic Hospital.

[The writer considers the subject under two heads, 1st. The disease as found in young infants; 2nd. As in children and young adults. At no period of life are operations so well borne, or so successful as in infancy. Each plan of operating seems nearly equally safe. In cases where the entire lens is opaque, it may be removed at once through a linear opening in the cornea, or it may be broken up and gradually absorbed in the aqueous chamber.]

In December 1860, a little boy, aged 2, with congenital cataract involving the entire lens, was brought up from the country, accompanied by a sister, aged 18, also suffering from partial congenital cataract, the cataracts were dense and only permitted perception of light. Under chloroform I made linear openings at the outer part of the cornea, to the extent of about two lines and a-half, then carefully, but freely opening the anterior capsule, using caution not to wound the posterior capsule, or enter the vitreous chamber, the cataracts were removed gradually by two or three introductions of a flat silver spoon; on opening the capsule, the nucleus escaped into the anterior chamber, and

was removed first, then the cortical portion, leaving clear pupils. Rapid recovery took place with small central pupils, and with excellent vision, and for days the little fellow was kept in a constant state of excitement in gazing for the first time upon surrounding objects. If the case could have been watched for some months, the same results might have been obtained by the old method of solution, and perhaps as a rule a more perfect result is obtained by this method in regard to the mobility and central position of the pupil; but care is required to renew the operation from time to time until all the lenticular matter is absorbed, and to avoid the formation of a tough opaque capsule within the field of vision. In infants, the lens may be very freely broken up and large portions may escape into the anterior chamber, without setting up any serious reaction or inflammation. I have even known the lens escape from its capsule, and become dislocated into the anterior chamber, and gradually undergo absorption, without producing any unfavourable consequences; this state of things would produce serious inflammation in an adult. In those cases in which the opacity is limited to the nucleus of the lens, some modification of the treatment is necessary; these cases differ in the extent to which the nucleus is involved, and in the degree of density in the opacity—usually in infants the opacity is well marked; these cases may be treated upon the same principles whether they are observed in infancy, in childhood, or in adult age.

We have now to consider the subject of treatment in partial congenital cataract, whether it comes before our notice in infancy, in childhood, or in adult age. In infancy, as I have before remarked, the opacity is usually strongly marked and dense, however it may differ in extent, and if it be of sufficient extent to occupy the pupillary area, when the pupil is in its normal state, it will seriously interfere with vision, and require some operative proceeding. There are three methods that may be adopted in such cases:—1st, to remove the cataract by solution; 2ndly, to perform linear extraction; 3rdly, to make an artificial pupil. The first of these methods is so well understood, and has been so long, and so scientifically practised, as to require no comments here.

The other two methods seem to me to merit some observations.

In the performance of linear extraction for partial congenital cataract, it is desirable that the adhesion of the lens to its capsule be loosened, that the transparent portion of the lens be made opaque, and that the general consistence of the entire lens be reduced, and its substance be rendered more diffluent. The principles upon which this operation may be most safely, quickly, and completely performed, have been carefully worked out and practically illustrated by my friend Mr. Bowman. His method is as follows:—The pupil having been completely dilated by means of a strong solution of atropine, the capsule is freely opened with a fine needle, and the lens is thoroughly broken up by repeated small strokes of the needle in every direction, at the

same time the utmost care is taken not to wound the posterior capsule, or enter the vitreous chamber. After this operation, the pupil must be kept well dilated with atropine. The effect of this part of the operation is to render the entire body of the lens opaque, to increase its bulk, and to soften it by dilution with aqueous humour, and to separate it from the capsule. It also sometimes causes a bulging of some parts of the broken lens through its capsule into the anterior chamber, which may be followed by more or less rather severe reaction. After a period varying from three to six days, and regulated somewhat by the amount of the reaction, and by the change produced in the substance of the lens, a linear opening is made at the margin of the cornea, and with a small spoon, the lenticular matter is gradually removed leaving a clear pupil. Atropine must again be freely employed to keep the pupil well dilated, until all irritation has passed off. If these various steps are carefully and skilfully executed, a very good result is almost invariably obtained. The advantages of the proceeding are, the rapidity with which the cataract is removed, and the avoidance of any of the inconveniences attending gradual solution, such as, considerable delay, slow inflammation, opaque and tough capsule, closure of pupil, &c. The risks attending the operation are, severe pain and reaction, threatening the safety of the eye, and a distorted and adherent pupil. I think it safer to make a tolerably free linear opening, rather than a small one, at the time the extraction is performed. I have certainly seen several cases most successfully treated in this way, and I think that both theoretically and practically the operation seems scarcely capable of any further improvement. I have frequently tested it, and have obtained favourable results, at the same time the impression left on my mind is, that in cases where time is no object, a more perfect result may be obtained, and inflammation may be more completely avoided, by the old method of solution; in this way a small central bright moveable pupil is obtained, securing the utmost amount of vision, that the eye when deprived of its lens is capable of. The third method to which I am anxious to draw attention is by the formation of an artificial pupil. This method is comparatively recent in this country, and its merits can only be determined by further experience; in the meantime it may be useful briefly to sketch the history of the operation up to this time, the cases to which it seems most applicable, the objections to the proceeding, and the advantages it offers as compared with the removal of the lens.

I believe Professor Graefe of Berlin was the first to make an artificial pupil in cases of congenital cataract, sometimes in combination with linear extraction, and sometimes as a single operation. His method was by iridectomy; I have no details of his operations, but I have reason to believe he was satisfied with the results.

Within the last twelve months, Professor Pagenstecher has performed iriddesis in several cases of partial congenital cataract; he considers this method preferable to iridectomy, and has indicated certain

points that he thinks important to be carried out in the performance of this operation. Thus he advises that the opening should be made in the sclerotic, just anterior to the ciliary ligament, so that the natural plane of the iris be preserved; also that a considerable portion of the iris be drawn out and tied, so as to draw the entire pupil near to the margin of the cornea, and thus cover as much as possible of the opaque part of the lens with the iris, and leave a well-defined small moveable pupil opposite to the transparent part of the lens. This plan, he contends, gives the best result optically, and is easily accomplished. I have performed this operation in three cases.

The first in November, 1860, Master B., aged 8, was brought to me with symptoms of short and defective sight, both for near and distant objects; cannot face the light; puts himself into peculiar attitudes when reading, and in twilight and candlelight seems nearly blind; he reads No. 14 of Jaeger with difficulty; his sight is not much improved by atropine unless No. 16 convex is employed, and by its aid he reads No. 10, he also sees distant objects better when atropine is used. With the ophthalmoscope partial congenital cataracts could be distinctly seen, rather more than half the centre of the lens was opaque, the margin being quite transparent. Under chloroform I performed iriddesis downwards and inwards, making the opening in the sclerotic and drawing out as much of the iris as I could through the opening, so as to make the pupil small and near the margin, and therefore opposite the transparent part of the lens, and at the same time to cover the remainder of the lens as much as possible; this object I accomplished more completely in one eye than the other, no irritation followed, on the second day the silk tie came away, and within a week the eyes were quite recovered. Sight was very much improved both for near and distant objects; could read No. 4 at 7 inches, or at least spell the words at that distance; and a few weeks ago I heard from his mamma to say that she found that her boy could distinguish distant objects as well as her other children, and that he was able to continue his education in a satisfactory manner.

The next case was a young female, aged 19, who brought her little brother to the Ophthalmic Hospital with congenital cataracts, and whose case I related at the early part of this paper. She was found to be suffering from partial congenital cataracts, in the left eye the opaque lens was partly detached at the lower part and sides, and swung backwards and forwards in the vitreous chamber. The sight was very defective in this eye, there being little more than perception of light, and the ophthalmoscope shewed evidence of changes in the choroid and retina, all of which were probably intra-uterine. The other eye when under the influence of atropine shewed a partial opacity of the lens with a clear margin, which margin seemed broader at the lower part than at the remainder of the circumference; her sight was very imperfect both for near and distant objects, and was not improved by either concave or convex glasses. She could read No. 14 of Jaeger

with difficulty at four inches distance, and could perform coarse needlework, but could never thread her needle. Her sight was somewhat improved by the atropine, but not to any great extent. Under chloroform I made an artificial pupil downwards in the same manner as in the last case, the eye rapidly recovered, and before leaving the hospital the patient could thread an ordinary needle, and read No. 6 of Jaeger at an average distance. The third case was in a little girl, aged 6, who was first brought to me on January 15, 1861. She was very myopic, and intolerant of a strong light. She had been taught to read with letters the size of about 18, Jaeger, and could not read 16. Her sight was much improved by atropine. Partial congenital cataracts were distinctly visible, the margin quite clear and appearing to be broadest below. On the 29th of the same month I performed iriddesis downwards in both eyes, and the result was most satisfactory, perhaps rather more so than in either of the two other cases. A fortnight after, this little girl could spell with rapidity the words of No. 4 of Jaeger, and could see distant objects well. The appearance of all these cases, after the operation, was very similar, and under the ophthalmoscope was rather singular. In all these was seen a small active balloon-shaped pupil; about one-third of the greater extremity of the pupil presented the opaque part of the lens, the remainder a perfectly transparent circumference up to the very edge of the lens, beyond which a thin transparent line of illumination was visible; through the extreme boundary of the cornea the fundus was clearly visible.

The advantages of this method, as contrasted with the removal of the cataract in partial congenital cases, seem to be, the extreme simplicity and safety of the operation, the certainty with which the size and position of the artificial pupil can be regulated, and the preservation of the lens, together with the natural focusing power of the eye, rendering unnecessary the use of glasses. In cases also where the opacity is so faint, and the sight so slightly impaired, as scarcely to justify removal of the lens, iriddesis seems to offer a satisfactory compromise. The only objections to the proceeding that suggest themselves to my mind are, the possibility of the margin of the lens becoming subsequently opaque, thus rendering the operation futile; and again, the transparent margin being in some cases so narrow, that the improvement to vision is very slight. In the event of the former, viz., the supervention of opacity of the margin, it would still be competent to remove the cataract under quite as favourable circumstances, as if an iriddesis had not been performed, and according to some high authorities even more so; and as regards the latter, the success of every proceeding must necessarily depend upon the judicious selection of cases. In young people the power of dispensing with cataract glasses seems to me a great advantage, and after weighing every point, I am drawn to the conclusion, that the application of iriddesis to properly selected cases of congenital cataract, is an improvement in ophthalmic surgery.—*Oph. Hospital Reports, April* 1861, *p.* 137.

84.—ON CONICAL CORNEA, AND ITS TREATMENT BY OPERATION.

By W. Bowman, Esq., F.R.S., Surgeon to King's College Hospital, and to the Royal London Ophthalmic Hospital.

Conical cornea is a thinning and bulging of that structure in its middle region, coming on imperceptibly, and altering or destroying the natural focus of the eye. Up to this time its anatomical conditions have hardly been fully ascertained, and though many optical and operative correctives have been applied to improve vision, occasionally with some success, there is room for further efforts in the same direction.

Its Anatomy.—The following is the only case in which I have had any opportunity of accurate examination. A young lady had had from infancy a slight prominence of one eye, and at 13 it had so increased as to excite alarm. A firm elastic tumour could be *felt* above the outer canthus, projecting the eye, which saw well. The tumour was punctured, fluid let out, and the eye receded, but in two years the eye had become once more prominent, and had now assumed the state known as conical cornea, remaining otherwise healthy. In three months more the tumor had thrust the globe more forwards, everting the upper lid, and almost destroying sight, apparently by pressing upon and stretching the optic nerve. The conicity of the cornea was very considerable, and as is usual in such cases was accompanied by opacity at the irregular apex of the cone. It was now necessary to remove, if possible, the whole disease. In company with Professor Syme I first excised the globe, then, partly through the space obtained, and partly by an incision below the brow, I succeeded in completely removing a very large sebaceous cyst, with bony plates in its wall, which was adherent to the very bottom of the orbit. The removal of a V-shaped portion of the lid then corrected the ectropion. The recovery was most satisfactory, the young lady now wearing an artificial eye.

Mr. Hulke gave me the following account of an examination I requested him to made of the cornea:—

"*Note on the perfectly fresh cornea.*—The cone is a very prominent one, and is confined to the central region. The apex is nebulous, but elsewhere the cornea is quite transparent. Its surface is polished, and not facetted. The aqueous humor, iris, and lens are normal.

"*Microscopic examination.*—A section was carried through the line of the ora serrata, so as to remove the anterior ring of the sclerotic, with the ciliary processes, the iris and cornea entire; this portion was then pinned out flat on a piece of wood, with the iris downwards, and dried. Various sections were then made, and moistened with water or acetic acid.

"The central conical nebulous portion was much thinner than the transparent periphery of the cornea, where the curve was natural. This thinning began at the base of the cone, and progressively increased towards the apex, where it reached its maximum. At this point the mean depth of several vertical sections was only one-third of that of the peripheral region. The continuity of the anterior elastic lamina was perfect, but upon the cone this structure was much thinner than elsewhere, and wrinkled; it was underlaid by a stratum of crowded, elongated, club-like nuclei, and beneath these the normal lamellar tissue was replaced by a web of caudate and nuclear fibres amongst the meshes of which clusters of large oval and fusiform cells were packed. The structure of the transparent peripheral region was perfectly normal, and at the base of the cone there was a gradual transition from the healthy to the diseased tissue, the interlamellar corpuscles becoming more plentiful, branched and drawn out into fibres, which in many instances coalesced with those from neighbouring corpuscles. The posterior elastic lamina and the epithelium, both on the front and on the back of the cornea, were unchanged.

"The changes I have described," adds Mr. Hulke, "were confined to the laminated tissue of the cornea and the anterior elastic lamina. The substitution of a web of nuclear fibres and cells for the regular lamination of the cornea, explains the nebulosity of the cone, and the liability to bulge."

The above account is quite conformable with what might have been anticipated from such an examination, and sets at rest doubts which may have existed as to the condition of the bulging tissue. It may remain a question in what degree the cell growth in the opaque part may be regarded as a cause, and how far as an effect, of the stretching.

Contribution to the general history of conical cornea from my own observations. (1.) I have observed a very few cases in which it occurred in more than one member of the same family. The usual age at which it commences is from 15 to 25, but it may begin earlier, and as late as 50. (2.) It is very often, but not always, attended by signs of delicacy of constitution, such as an hereditary tendency to pthisis, over-rapid growth, a weak digestion, feeble circulation. (3.) It very commonly occurs in both eyes, sometimes simultaneously and equally, more often unequally, so that vision is worse in one eye. I have seen several cases in which it has affected only one eye, but in these it has with very exceptions remained slight. One man of 60 had an extreme degree of it in one eye, none in the other. (4.) It may become stationary at any degree, from such a slight bulge as can be only detected by the ophthalmoscope up to an extreme irregular bulge, and opacity destroying the sight of all but the largest objects. (5.) It never advances to bursting either by extreme stretching or by ulceration or sloughing. (6.) It always proceeds gradually but with variable speed:

when come to a stand it sometimes, though rarely, may take a fresh start. (7.) When the tendency exists, it seems to be aggravated by whatever strains the eye; consequently over-use and everything inducive of congestion is to be avoided; local fulness is to be combated by the usual remedies, while the general strength is to be upheld by all suitable means. (8.) In many cases, especially those of a slight and slow progress, there is no mark of weakness or excitement in the circulation of the eye; in others the ciliary vessels are overcharged, often perhaps from imprudent efforts to use the organs when sight is failing. (9.) Soon after the immortal invention of Helmholtz, I found the ophthalmoscope very useful in detecting slight degrees of conical cornea. For this purpose the concave mirror only is to be used without a convex lens. On turning the mirror so as to throw light at different angles, the side of the cone opposite to the light is darkened. Probably every one is now familiar with this test. Professor Donders told me last year that he had himself observed it. It is a very useful one in discriminating the cause of slight defects of vision, somewhat resembling myopia and hitherto deemed anomalous. I have seen many such cases. (10.) As to the physical state of the eye, I have observed that there is no undue tension of the globe appreciable by the touch; in some instances I have even thought the tension rather less than natural. In these examinations I have endeavoured to distinguish between the tension of the vitreous and aqueous regions of the eye, as I conceive it is desirable to do in reference to all the diseases in which the ocular tension comes under consideration. (11.) The pupil is apt to be large, and sometimes the iris inclines to be tremulous.

Rationale of the Disease.—It seems certain that the bulging of the cornea is due not to an increase of the normal intra-ocular pressure, but to a diminution of the resistance of the cornea itself. In the natural condition, the cornea is sufficiently strong to maintain its true curvature under the ordinary tension; and even when this tension is augmented, and continues so for a long period, the cornea does not bulge, unless it has previously undergone visible change of structure, and rarely even then. The ciliary or posterior regions of the sclerotica bulge rather than the cornea. I conclude this softening of the cornea to occur only in the bulging part, and not near the margin. At first the softening is not attended with such alteration of the anatomical elements as to produce opacity; but as the bulging increases this alteration does take place; either as a further stage of the same process of softening, or as a consequence of the stretching of the softened part.

It has often seemed to me remarkable that the bulging never goes on to bursting. The reason, probably, is, that as the cornea becomes thinner, the escape of the aqueous humor by exosmose is facilitated; and thus the internal pressure is reduced so as to be no longer in excess of the diminished resisting power of the cornea. A balance

is re-established like that of health, only that there is a more than ordinary outflow of the aqueous humor by transudation through the cornea. This accords with my previous observation, as to such eyes being rather unduly soft. I do not pretend to explain why the cornea should thus occasionally tend to become less resisting and tough in its central region. Is it the consequence of some undefined change in the chemical constitution of the lamellated tissue? I can hardly venture to assign it with any plausibility to a primary alteration in either the anterior or posterior elastic lamina. Certainly the membrane of Descemet is not strong enough to bear any important part in the production of this disease; and as for the anterior membrane, I have many times extensively removed it with even some of the lamellated tissue without observing the slightest after-tendency to conical bulging of the cornea.

Treatment.—No reliable testimony exists as to the success of any measures hitherto adopted for the restoration of the cornea to its natural curvature, though, possibly, it would be wrong to say that that is not an object to be aimed at in slight and early stages.

Indications for the treatment by arrest.—These are two, to check the softening process, and to diminish the intra-ocular pressure. As to the former, in the imperfect state of our knowledge, we can only pursue measures of a tonic kind, which, indeed, are very commonly indicated by the general symptoms. When there are signs of congestion it is most important to meet them by the cold douche, by very moderate and cautious local abstraction of blood in certain cases, by abstinence from all that kind of use and exposure of the eyes which excites and reddens them, and from shedding tears.

My friend, Professor v. Graefe, has, within the last three or four years, discovered the important fact that the excision of a portion of iris diminishes the intra-ocular pressure, a fact of pregnant interest in many diseases of the eye, and which he has applied with consummate skill to the treatment of glaucoma, of certain staphylomata, and last autumn to that of conical cornea. While agreeing that when this latter disease has attained a certain degree, and a certain age, all medical treatment is useless, he gives a case in which he practised iridectomy with the double object of favourably modifying the position of the pupil, and of lessening intra-ocular pressure, and he thinks with good results in both respects. (Archiv fur Ophthal., vol. iv., part ii., p. 271.) The history of operations undertaken in cases of conical cornea is interesting; but up to this time none has come into general use. Mr. Wardrop repeatedly tapped the aqueous chamber. Sir W. Adams removed the lens by solution. Mr. Tyrrell drew the pupil outwards and downwards with the blunt hook, excising the prolapsed portion of the iris. If the iris then remained in the puncture, the pupil must have been balloon-shaped, and probably useful; but if it receded —an equal chance—the pupil would be a large irregular one, partly opposite the margin, and partly behind the central bulge

of the cornea, and probably of little value. This operation has been the only one yet attended with any good result. Mr. Tyrrell himself tried it in several instances with advantage; in two with great improvement of sight. His great authority led surgeons to repeat his suggestion, but with variable and conflicting results, and I believe the proceeding has never come into common use with any surgeon. One eye was lost by destructive inflammation. In two the confusion of vision was aggravated. Mr. Walton has several times performed it without the slightest benefit. Mr. Wilde modified it by refraining from the excision of the prolapsed iris, and with marked advantage. Mr. Cooper and others had also variable, and on the whole, unsatisfactory results. (See a good *resumé* in the American edition of Lawrence on the Eye, by Dr. Hays, p. 393.) At present, I believe it may be safely said that operative proceedings in conical cornea have been generally abandoned as hazardous and uncertain. Some patients have been unwilling to submit to an operation when the surgeon could not promise a definite advantage, and the surgeon himself has often preferred the less formidable contrivances devised by the optician which have been frequently found of great use. In fact, at Moorfields, I hardly remember that any operation for the relief of conical cornea had been practised of late years until it occurred to me in 1857 to attempt to modify the pupil by a new method.

It has long been known that an eye thus affected often sees much better when either a small hole or a narrow slit in an opaque diaphragm is placed immediately before the eye. This is owing to the interception of all but a narrow pencil or plane of rays, which passing either laterally or centrally, form a more or less true image, unconfused by admixture with others less accurately converged, entering through neighbouring and more distorted portions of the corneal surface.

Such a contrivance has to be applied with a certain amount of skill, and to be moved as the eye itself moves, and it is seldom useful except for very near vision, because the area of the field is so contracted. To be useful, the aperture must be brought very near the cornea, and on the whole, though often far better than nothing, it is ill-adapted to poor and uninstructed persons. The idea on which I proceeded was so to operate on the iris as to give the pupil a slit-like shape, and to fasten it to the cornea in such a position as to conduce to the formation of the most correct image compatible with the altered curve of the cornea in each particular case.

It seemed likely that if the slit-like aperture could be placed within the eye, immediately behind the cornea, it would be more useful than the artificial perforated diaphragm placed outside. Without confining myself, however, to this idea, I have been endeavouring for the last two years to discover experimentally what modification of the pupil artificially produced was most effectual in improving vision. In no case have I had reason to regret having operated; no case has been made worse;

all, with one exception, have been improved, and none has caused anxiety by inflammatory threatenings consequent on the operation.

The first case I shall relate is that of Sarah Collins, aged 26, who for eight years had been delicate, and for six years had suffered from conical cornea, both being equally prominent and both transparent. With this exception, the eyes were quite natural, she could with an effort read the smallest type for a few moments if held very close, but had been compelled to abandon her employment on account of the impossibility of seeing the objects around her.

On April 30th, 1858, I operated on each eye in a different manner, but with the same object, viz., that of drawing the pupil outwards to a fixed point at the margin of the cornea, as the first step towards forming it into an horizontal slit. On the left eye I excised a minute marginal portion of the cornea by the help of a broad needle (*corneëctomy*—the operation of Guepin modified) a small portion of iris was immediately protruded and the pupil became transversely balloon-shaped, the margin of the pupil not being involved in the hernia. On the right eye I performed the operation of *tying the iris*, then just devised by my friend Mr. Critchett, to meet the want we had long felt of a sure method of fixing the iris to a point of puncture in cases of artificial pupil (see Ophthalmic Hospital Reports, vol. i, p. 225, Oct. 1858.) In both eyes the canula forceps was used to seize and draw out the iris to the degree desired, the point seized being about one-third from the pupillary margin, so as to leave the margin still free within the aqueous chamber. The object in each eye was attained, but with so much less irritation from the iriddesis that in all subsequent operations I had recourse to this method, which I regard as one of the most useful improvements made of late years in the operative surgery of the eye, giving as it does precision and certainty, with perfect security under all ordinary circumstances, to the operation for artificial pupil, in the very numerous cases to which it is applicable. The sight being now much improved, but most so in right, I operated inwards, or at an exactly opposite point, in both eyes, on the 21st of May, the immediate result being a transversely oval pupil in each. In the right eye the margin of the pupil was entirely free; in the left, the margin was adherent to the site of the corneal wound. Hardly any irritation ensued. At the end of two days the ligatures had dropped off; the pupils acted promptly, the left being more slit-like than the right. In both eyes vision was immediately much improved, and a gradual further amendment has since gone on, apparently owing to a certain receding of the corneal bulge.

This girl has become able to resume her occupation of a housemaid, and is able even without concave glasses to see the objects in a room with tolerable accuracy; with a glass she can see distant objects still better.

I have operated since on six other patients, on five of them in both

eyes. I have operated so as to make the pupil vertical rather than horizontal in all these cases, and in several have experimentally modified the operation in regard to the point of the iris seized by the canula forceps, and so as to test the value of that instrument, compared with the short blunt hook for seizing the pupillary edge. This last I now prefer, as being simpler and less apt to injure the iris, while it affords greater precision in fixing the pupillary edge to the wound. I find, too, no disadvantage in engaging the pupillary edge in the wound, the pupil acting as well afterwards as when the natural pupil is still entirely within the chamber. I have tried to perform iriddesis in the opposite directions on the same eye on the same occasion, but found that the point first tied was apt to be drawn unduly into the chamber while the opposite point was being drawn out, so that I think it better to do the second iriddesis on the same eye a week after the first.

In one of the earlier operations with the canula forceps there was a little bleeding from the stretched iris into the chamber, sufficient to disguise the later steps of the operation, but with the short hook this has never occurred. These operations from their extreme delicacy should be done under chloroform, the influence of which should be sufficiently complete to place the organ in complete repose; one minute is sufficient for each eye when dextrously managed. If sickness ensue it does no harm to the eye. No iritis suffusing the pupil has followed in any case, but of course the usual care has been taken to keep the eye cool and quiet for a few days after the operation. If the thread remains on after three or four days, it may be pulled away with forceps. It is of great importance to the perfection of the operation that the puncturing instrument and the blunt hook should be of equal size, so that a sufficient opening, but not more than sufficient, is made for the free passage of the hook; otherwise, either the hook does not enter easily, or, on the other hand, the knot is prone to slip back into the puncture, or even into the aqueous chamber. The recurved part of the hook should be long enough to hold the pupillary margin, but so short as to admit of the ligature being tightened the moment this margin is outside the puncture. I always leave one end of the silk nearly an eighth of an inch long, so that it may be readily laid hold of afterwards, should it chance to have fallen at all inwards, or be concealed by a coating of lymph. These may seem trifling points, but in my opinion have their importance, and are not unworthy the attention of any one performing this operation for the first time—they pertain to the excessive delicacy of the structures, and of the whole proceeding.

It has been a matter of great interest with me to ascertain, as far as the limited number of cases presenting themselves enabled me, how far the second iriddesis was useful, in other words, whether an elliptical or slit-like pupil gave better sight than one of a balloon-shape; and again, whether the vertical direction was better than the

horizontal, or the reverse; also whether other modifications in the direction of the altered pupil were desirable.

Of course, this is a subject which must be worked out in detail by surgeons, and the following remarks are but hints for future experimental enquiries.

The slit-like figure of the pupil suggested itself to me as the most feasible method of much limiting the size of the pupil while changing its situation. If the thing be well considered it will be found difficult to conceive any plan of rendering the pupil *very small* by operation; it is much more easy to *enlarge* it. While the pupillary margin is free, any displacement of the iris is most likely to enlarge the pupil, for I despaired of being able to seize the iris at one side of the pupil and draw and fix it over towards the other side. Any excision of the iris must, of course, enlarge the pupil, and so must any marginal iriddesis; but by a double iriddesis in opposite directions the central region becomes slit-like. By making this slit *horizontal* the light is admitted from each side, as well as through the centre, and the range of the field would probably be expanded, while the precision of the image would be impaired by the inequality of refraction through the central and marginal regions respectively. The appearance of the horizontal pupil was besides not agreeable. The *vertical* position of the slit offered the prospect of its virtual reduction in size, by the overlapping of its angles by the lids in ordinary vision; and I hoped that the patient would learn to use the lids for this purpose, so as to clarify the image. I cannot say that this expectation has been borne out in any marked degree, but nevertheless the vertical slit is much more sightly than the horizontal, and certainly equally good for vision, so that at present I prefer it.

The improvement of vision from a first iriddesis downwards has been in almost every case decided, the patients being delighted with the result. In some the second iriddesis upwards has not seemed further to increase the precision of view, in others it has certainly done so, and in the present state of the enquiry I am disposed to continue to practise it in cases of considerable conicity. The improvement, however, consequent on the second operation, is never so marked as that which follows the first, and it can only operate by narrowing that part of the pupil which lies behind the bulge. Its more or less influence in different cases may perhaps depend on varieties in the curvature of the apex of the cone.

The influence of these operations in lessening the corneal bulge has been very remarkable. It is not easy to give in any case the exact amount of this result, but that the bulge diminishes speedily, and continues to do so for a considerable time subsequent to the operation, admits of no doubt, and I attribute it to the moderation of the ocular tension. Further experience will show whether I am right, but at present I am for operating quite early in slight cases in a downward direction only, if only to arrest the progress of the conicity; and cer-

tainly to obtain this result in almost any degree will make it worth while to perform so safe an operation in a disease otherwise so intractable, and, in its advance, so destructive of all useful sight.—*Ophthalmic Hospital Reports, Oct.* 1859, *p.* 154.

85.—A NEW AND EFFECTUAL CURE FOR ENTROPION AND TRICHIASIS.

By Dr. JOHN WILLIAMS, Surgeon to the Cork Eye Infirmary.

Few diseases of the eye prove more distressing to the patient or more trying to the surgeon than those in which the lashes are inverted or misdirected.

The friction of the inverted ciliæ keeps up constant irritation of the eyeball, and unless the disease be interfered with by operation, the cornea becomes clouded, thinned in its structure, and finally gives way; total blindness or considerable impairment of vision resulting.

With a view to obviate such effects, many operations have been proposed by Crampton, Jæger of Vienna, Jacob, Wilde, and others, which it is not my present intention to refer to; but, unless the bulbs of the lashes be *completely excised*, it will almost invariably be found that any other operation affords but temporary relief, and the surgeon will, after a time, have inverted ciliæ again to contend with.

Cork has gained an unenviable notoriety for inverted lashes and granular lids, and my experience proves that this reputation is unhappily too well deserved; the number of those cases being about sixty per cent.

These scrofulous diseases are almost exclusively confined to the very poorest classes in Cork, among whom scrofula prevails to a great extent, and I may remark in passing, that the non-recognition of granular lids as a *scrofulous* affection—which I believe it to be—and the dependence on local applications alone, to the exclusion of constitutional remedies, in its treatment, have added considerably to the difficulties of cure.

The plan of treatment which I now beg to bring before the notice of the profession is, although simple, perfectly efficacious, and is of great value in those cases in which we would desire to save, if possible, the natural ciliæ, and destroy those only which, by their inversion or misdirection, were producing on the eyeball the ill effects before referred to. These cases are:—

1st. Where a whole row of supernumerary lashes lie interior to the natural row of ciliæ.

2ndly. In those cases in which a few only—perhaps a single hair—of the *natural* lashes are inverted; the others maintaining their normal direction.

3rdly. Where a few lashes, either isolated or in tufts, are scattered along the tarsal border.

Another great advantage my plan of treatment affords is the preservation of, in most cases, the meibomian glands; for, as Mackenzie remarks, *lippitudo* follows the operation in which the tarsal border, including ciliæ and meibomian glands is clipped off.

I will now proceed to describe the treatment I practice.

Having placed a small piece of potassa fusa in a porcelain capsule or bottle, and adding a drop of water to hasten its deliquescence, I dip the point of a fine needle—or a small knife, if the inverted lashes be in tufts—in the *liquid caustic potash*, and then plunge it into the tarsal margin along the course of the hair or tufts of hair, to the depth of the eight of an inch, at which depth the hair bulbs lie (I would recommend a Desmarres ring-forceps or horn spatula to be placed under the lid for the protection of the globe, and better performance of the operation). The pain attendant on the puncture is trifling, and may be instantly removed by applying cold water with a small brush to the border of the lid.

On the second or third day the lashes thus treated are removed with forceps; they are pulled out without any difficulty, and are blackened towards the roots. The bulbs of ciliæ are completely destroyed, never to be reproduced. Unless an entire row of lashes is to be removed no inflammation follows the operation; even in this case it is of little moment.

For the removal of nævi, fungous growths, small tumors and *warts* in *any* locality, punctures with a needle or small knife dipped in the liquid caustic potash will be found useful, as the needle or knife burns along its subcutaneous track. In the above remarks, I have made no allusion to the amount of deformity left after the loss of the eye-lashes, as I consider it of no moment when the preservation of vision is effected. —*Dublin Hospital Gazette, Jan.* 15, 1861, *p.* 18.

86.—ON RECOVERY OF THE LOWER LACRYMAL PUNCTUM BY A NEW OPERATION.

By J. F. Streatfeild, Esq., Editor of the Ophthalmic Hospital Reports.

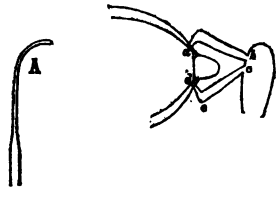
[The case was one of very old standing tinea tarsi, in which, as usual, the lashes were nearly all destroyed. The lower punctum of the right eye was everted and lost in the thin tense skin of the lid. By appropriate treatment the condition of parts became much improved, but the tears constantly overflowed the lids, and the first indication became to reinstate the lower tear duct.]

According to the anatomy of the parts I thought I might be able to pass a probe from the upper canaliculus into the lower one, the upper canaliculus was already slit up, and this would facilitate the use of the probe. I did not think that the lower canaliculus was likely to be obstructed or diseased, although the punctum was lost. The

course I proposed to take with the probe was all by soft parts, which would readily yield, and if I could keep the probe in the right course throughout and avoid entangling its point in any fold of mucous membrane, I thought I might be able either to pass the probe's point out at the place of the lower punctum, or at any rate, by feeling with the finger, detect the probe's point, and cut down upon it through the mucous membrane. I did not believe in the valves which have been supposed to exist at the extremities of the canaliculi towards the lacrymal sac, and I knew that exploratory operations (incisions made by the conjunctiva, &c.) for the discovery of the lost passage had not been successful.

On the 22nd, when she came again, with the eyelids of the two eyes improved, but unequally, I took one of Mr. Bowman's canaliculus styles, and bent the end of the thin part into a small curve (A. in the figure) and just the point of the part curved (turned towards me) bent a very little to my right hand. I chose this canaliculus style instead of a probe, because I was sure the thin part of it would be long enough for my purpose, and the thick part I thought would be more convenient to be held in the fingers and more easily directed than a very fine probe such as I should (hoping to be able to pass it out through the lost punctum), have been obliged to select for the attempt.

I took the thick part of my instrument, and holding it downwards close to the patient's cheek in a line with the nose, passed the point of the bent thin part into the upper canaliculus, passed it along it without moving the position of the handle, and pressed it inwards until I was sure the point must be in the sac, then I brought the handle round towards the temple, making the point of the instrument keep, as well as I could judge, in the direction of the outlet of the lower canaliculus by an abrupt turn, according to the anatomical relation of the parts (*a*, *b*, *c*, *d*, in the figure), to reverse the direction of the point (carefully avoiding any force, and, when the point seemed to be caught, moving it about a little) then the handle being brought over the brow of the patient, I gently pressed the point along the lower canaliculus, which with very little difficulty I found I had entered. I could feel the point of the instrument distinctly through the conjunctiva, but could not bring it out through the punctum at first, and thought of cutting down on the point, but by bringing the handle more across the root of the nose between the eyebrows, and manipulating a little with a finger the skin of the lower lid, the point showed at the punctum, and with the



A. the instrument. *a*, *b*, *c*, *d*, the direction it was made to take.

pressure of a finger it emerged, then I simply divided the lower canaliculus to some extent, upon the probe, withdrew it and ended in the regular course, by passing in at the opening I had made, along the remains of the canaliculus, and down the nasal duct, No. 6, (the largest) probe.

On the 29th, I found the patient's state rapidly improving. The escape of the tears was perfect, the sore edges of the lids were nearly healed, the natural appearance of the skin was restored and the ectropion was consequently almost cured. Her appearance and the state of the parts was, soon after this, as much improved as they could be, for her eye-lashes were at first destroyed.

On November 5th, the lippitudo was less conspicuous, and so she was doing well. This day I practised again with the bent style, the "tour" I made on the 22nd, this I did in order to learn how it should best be done. I was surprised to find that, as she sat in a chair, I could not do it at all, this I could only think was on account of the twitching of the eyelids, which she could not control (the orbicularis and tensor being in constant movement whilst I used the instrument), and it appears to have been so, for when she was put under the influence of chloroform (which on the former occasion she had taken) I, with very little difficulty, passed the bent style from the upper punctum and canaliculus, by the sac, through the lower canaliculus and punctum. In passing the bent style I found it was best to press the convexity of the curve rather upwards in traversing the upper, and downwards in the lower canal, thus more probably keeping the point of the style free from entanglement. The knowledge of when to make the sharp turn to (avoid the nasal duct and) enter the lower canaliculus, I gained by passing the point inwards towards the nose till I felt the bone, then drawing the point a little back, I rotated it as on this point, and directed it first down and outwards, and then more outwards. I never used any force with the point of the instrument, but when the bent style has been in the way here described passed into the lower canaliculus, of course a fold of skin of the upper lid rests against the handle of the instrument.

In order to learn that I was able, if in any case in future it might be desirable, I afterwards on this occasion, passed my bent style once in the contrary direction to that I had been practising (also with little difficulty) that is to say from the *lower* canaliculus, by the sac, and out at the upper punctum.—*Ophthalmic Hospital Reports, No. 12, p. 5.*

87.—*Case of Injury to the Eyes.* By F. Le Gros Clark, Esq., Surgeon to St. Thomas's Hospital.—[The following case occurs in a lecture by Mr. Clark, and is interesting from the comparatively trifling ill consequences resulting from so severe an accident.]

J. B., aged 30, an engineer, was admitted into the hospital on September 24, 1860. The patient was engaged in executing some repairs to a steamer, and was using some melted metal—a patent mixed metal, which melted at a temperature of about 80° above the melting point of lead. The accident occurred by the metal splashing, and a quantity of it was thrown up into his eyes. When brought to the hospital, both his eyes were found to be filled with fragments of a metal resembling lead, diffused over, and adhering to, the conjunctiva and cornea, as well as to the eyelids. The eyes were cleansed, as far as practicable, by means of a probe and camel-hair pencil; and, oil being dropped into them, the patient was removed to bed. Some shreds of disorganized membrane were also removed with the fragments of metal. A purge was ordered by the house-surgeon, and six leeches to each temple. Subsequently, several more fragments of metal were removed. On the following day he complained of acute pain, and the vessels of the conjunctiva were injected. On the third day a careful examination of the interior showed the ocular conjunctiva to be clear and free from ulceration, and only slightly injected; but the outside of the lids, which had been burnt by the metal, exhibited patches of ulceration. In the course of the following week he left the Hospital well.

I did not see this patient when he was admitted, and was surprised to find so limited an amount of injury, after hearing the history of the case, and ascertaining the temperature of the metal. One of you remarked to me that you had seen workmen plunge their hands, when recently wetted, into molten metal with impunity; and the explanation suggested at the time is that the sudden conversion of the water into steam protects the hand from injury. Such may be the explanation in this and similar cases, where certainly we should anticipate more serious mischief, and the eye escapes almost uninjured. In comparing such an accident as I have described, with the effects of the contact of unslaked lime or mineral acids with the conjunctiva or cornea, the contrast is very striking. In the latter case the destruction is immediate, and the opacity of the cornea permanent and irremediable; a point of considerable importance to be borne in mind in the prognosis you give.—*Med. Times and Gazette, Nov.* 10, 1860, *p.* 488.

88.—ETHER AS A REMEDY FOR DEAFNESS.

The attention of the public, both medical and non-medical, has been of late attracted by a fact which, though in its origin and early phases going back some years, is of interest through the publicity given to it by the official journal of the University of Paris, as well as through the daily press.

To state the question briefly:—About the month of August, 1855, a certain Mademoiselle Cléret, a private governess, inhabiting a popu-

lous part of Paris, applied to the Minister of Public Instruction for assistance, basing her application, among other grounds, upon her knowledge of a method of causing the deaf and dumb to hear. This method, the discovery of which was accidental, and of which she had made a successful trial upon some pupils suffering from deafness, after having experienced its efficacy in her own person, consists in the use of sulphuric ether dropped direct into the external auditory canal, at the rate of four, five, six, or eight drops per day. After the application of the remedy for fifteen or twenty days, its use may be suspended some days, and then renewed: it may be continued, if not indefinitely, at least for a very lengthened period.

A commission appointed by the minister, and of which the medical element included M. Lélut as president, M. Béhier as secretary, and the late M. Bérard, was deputed to examine into the state of the children submitted to it by Mademoiselle Cléret. The commission pursued its investigation with the utmost diligence, until upon a sudden the lady was seized with a fearful malady. After having waited, without much hope, an improvement in the mental condition of Mademoiselle Cléret, the commission drew up its report, although the question, necessarily suspended, did not appear susceptible of being brought to a definite conclusion, or to a complete and demonstrative result. It considered, however, that it was its duty to make known such facts as it had witnessed: this it did in the following terms:—

Twenty-nine children have been treated by this lady; all with beneficial results. Two of those brought by her before the commission, and who had been treated by her previously, were completely cured. Seven children have been submitted to the commission previous to any trials being made upon them, and their absolute deafness and dumbness demonstrated by Mademoiselle Cléret; and in all cases, but especially in four, a manifest change has been perceptible after eight or nine months of treatment, and the patients have been able to recognize with great ease various sounds. The reporter to the commission has been careful to add that the most minute precautions were taken to avoid sources of error, and to guard against any illusion that might arise from perception derived through another sense than that of hearing.

The commission wishing to test the means used by Mademoiselle Cléret upon other cases than those exclusively under her own care, deputed one of their number to carry out her plan of treatment upon other patients. Twenty persons were accordingly entrusted to him; most of them were deaf-mute children, but there were also some old men whose hearing was impaired, in some cases upon one side only. In all these a very noticeable effect was produced. It was also found that patients whose sense of hearing had become impaired through typhoid fever were very speedily restored by the same treatment.

In conclusion, the commission state that, with the exception of two or three children, whose deafness and dumbness was attested by authentic certificates, and who now hear well, it has determined nothing but the incomplete results of experiments commenced but not terminated, which manifest improvement, but nothing more definite.—*Gazette des Hôpitaux.*—*North American Med.-Chir. Review, Nov.* 1860, *p.* 1101.

MIDWIFERY,

AND THE DISEASES OF WOMEN, ETC.

89.—ON RIGIDITY AND DILATATION OF THE OS UTERI IN LABOUR.

By Dr. CHARLES D. ARNOTT, Gorleston, Great Yarmouth.

[Dr. Arnott gives us the following remarks on this subject. We will first let him explain himself in his own words, and then offer a few comments.]

The following brief outline may serve as an illustration of a case often met with in practice. A patient is in labour with her first child, and has been so many hours. The pains have been regular, have gradually increased in severity, and have now reached such intensity, that the nurse and friends are assured the necessary assistance only is needed to effect speedy delivery. Upon arrival, the medical attendant can but be similarly impressed; the pains recur regularly, are strong, and present all the characters of the genuine expulsive efforts, the last in the train. Digital examination reveals the following conditions:—Vaginal passage dry and tight; os uteri barely within reach, high up posteriorly, a mere slit like a button-hole, scarcely admitting the finger's point; the fœtal head low down anteriorly, pushing before it a large segment of uterine tissue, and under the influence of each pain apparently making ineffectual efforts to force its way through this evident source of obstruction. Under these circumstances, inexpressibly unhappy, the labour, without adequate assistance, must inevitably persist for hours, perhaps even for days.

This is a correct delineation of those cases which are described as depending upon rigidity of the os uteri; but, as I apprehend, improperly so. In the great majority, the os is, in reality, dilatable enough, but some of the chief elements of dilatation are acting disadvantageously, or not at all. The fluid wedge, for instance, is generally absent, the membranes having ruptured early under the influence of the strong uterine contractions, and the fœtal head is expending all its force in a wrong direction, not upon the os uteri, as occurs under normal conditions, but upon that shelf of uterine texture it has infringed upon, and which so long must successfully resist it.

The treatment of these cases the teaching of the schools has hitherto inculcated, I am now fully convinced, is not only inert, but highly injudicious. Bloodletting and antimonial depressants (time-

honoured maids-of-all-work) have been loudly extolled as adjuvants of vital dilatability, and almost implicitly relied on. Anodynes (learnedly) to lull excessive irritability, (truthfully) a clumsy excuse to gain time, have also had their share of renown, and these again are now likely to be superseded by a more potent agent, Chloroform.

I shall not discuss in full the merits of these plans of treatment. I am, from repeated experience, as fully convinced as I ever can be of any one professional fact, not only that they are of no use whatever, but that they are positively and highly injurious. My opinion is confirmed, that the effectual treatment of these cases is by surgery alone: reduction and dilatation of the abnormally-placed os by manipulation, and in the more aggravated and resisting cases, incision of the opposing texture. Much good service may, indeed, thus be rendered, a great amount of maternal suffering saved, and infantile life preserved.

Free lubrication of the parts having been premised, the finger is to be introduced during each pain, and the os solicited downwards and forwards, and at the same time freely dilated without any unjustifiable force. As with the prostate after the deep incision in lithotomy, so with the os in the great majority of these cases, it will be found to yield readily. Improvement commences forthwith, and all difficulty is soon overcome. In the very rare minority, incision (mere notching with a guarded probe-pointed bistoury) may be requisite in addition to manipulation, and the most intractable cases are speedily made to assume a totally different and more promising aspect.

I am aware the proposal seems harsh and may be denounced by the *laisser aller* school as belonging to that confessedly bad category, "meddlesome" midwifery. I believe, however, it may be fully proved undeserving any such epithet, by the remembrance that there are cases which tax our resources to the highest extent, from the extreme anxiety generally pertaining to them, and the inefficacy of the means usually employed for their relief; and further justified by the great fact of its being merely a close imitation of nature's own *modus operandi* under these special circumstances.

Let us briefly inquire how this difficulty is ordinarily surmounted by nature's efforts when unaided? Almost always, as all observant practitioners have again and again experienced, by spontaneous laceration, sometimes rather extensive, of the resisting uterine tissues; and, in spite of bloodletting, antimonials, and all other such auxiliaries, as they are inaccurately termed, scarcely ever, until this is accomplished, can she complete the process. Instances in which very extensive rupture ensues, are, with this concomitant complication, not uncommon; and many so called cases of occlusion of the os uteri, in which incision is the only alternative, nature having failed in effecting the necessary laceration, are probably often little more than extreme cases of the description we are now considering, in which the os, being more out of reach than usual, escaped detection.

I have employed the practice now recommended for many years, and on many occasions I have satisfied myself of its invariable and great utility; moreover, having never observed one untoward sequence from its employment, I conclude it may be regarded almost entirely free from danger. Let it but have a fair, unprejudiced trial, and I feel assured it will so forcibly commend itself, as to assert its superiority over all other expedients.

[There is much truth in what Dr. Arnott here says, but we think that he has undervalued the power of both ipecacuanha, antimony, and chloroform. In a case of this description we quite agree with Dr. Arnott, that to introduce the finger into the os, and during each pain, or even in the intervals, gently to draw the anterior lip downwards and forwards so as to get the os as soon as we safely can into the *axis of the outlet* of the pelvis, instead of allowing it to remain in the axis of the *brim* of the pelvis, is sound practice; but this is wonderfully accelerated by placing the patient under the influence of chloroform. Instead of waiting for six or eight hours, which formerly we have been obliged to do, we can now get over this dilatation of the os by a little gentle dilatation with the finger, while the patient is very mildly influenced by chloroform, in a remarkably short time. In fact, by following this practice, we have been delighted often to get home at midnight, instead of next morning to breakfast, and this too with great benefit to the patient, and a great saving of misery. We would strongly recommend our friends to chloroform their patients in this way:—Get a small cup, put a little chloroform at the bottom, and let the nurse hold the edge of the cup under the lower lip of the patient, so as to let both mouth and nose be free. You may thus keep the patient gently affected for a long time, and you have no trouble about it. You are busy with the os uteri, and the nurse manages the chloroform, although you watch its effects carefully. Ipecacuanha and antimony will have a similar effect, but these are now banished from practice by the more pleasant and more efficacious adoption of chloroform.]—*Lancet, Nov.* 17, 1860, *p.* 485.

90.—ON THE OPINIONS OF WILLIAM HUNTER RESPECTING RETROVERSION OR RETROFLEXION OF THE GRAVID UTERUS.

By Dr. W. Tyler Smith, Physician-Accoucheur to St. Mary's Hospital.

[In an interesting paper on this subject read before the London Obstetrical Society,]

After giving an account of the way in which our knowledge of this displacement of the uterus has been acquired, and the opinions of ancient and modern authors, but particularly of William Hunter upon this subject, Dr. Tyler Smith proceeded to lay down his own

views, and especially to dispute the Hunterian doctrine that the chief and exciting cause of complete retroversion is retention of urine and distension of the bladder. He then went on to say: "My own attention became specially directed to the subject of retroversion of the gravid uterus in the following manner:—I attended a lady, a patient of Sir Ranald Martin, who, in the unimpregnated state, suffered from complete retroversion or retroflexion. She left this country, with the uterus retroverted, to join her husband in India. She soon became pregnant, and went the full time. The question suggested itself to me, What was the condition of the uterus in this case, after impregnation occurred? And I resolved to take any opportunities which might occur to me of answering it. I have now seen a considerable number of cases in which the retroverted uterus has become impregnated, and have carefully watched the progress of gestation under these circumstances. The result has been a conviction that the most common cause of retroversion of the gravid uterus is not to be found in the state of the pelvis, or the condition of the bladder, but in the occurrence of impregnation in the retroverted uterus, and in the tendency of the organ thus impregnated to grow and develop itself during the early months of pregnancy, in the retroverted or retroflexed position. When an ovum is deposited in the retroverted uterus, the enlargement of the organ causes a greater sense of weight and pressure in the pelvis than ordinary pregnancy. The os uteri approaches the pubis, and the fundus projects towards the hollow of the sacrum. The fundus is found to enlarge considerably when examined from time to time by the finger. At length, unless the pelvis is of very large size, the bladder and rectum are pressed upon so as to interfere with their functions, and difficult micturition and defecation, especially the former, are the results. Owing to the retention of the gravid uterus within the pelvis, there is little or no increase in the size of the abdomen. There is usually a great amount of pain and discomfort in the lower part of the back, and the sympathetic affections of pregnancy are frequently more severe than usual. Abortion very frequently occurs from the mechanical irritation of the uterus." He had published an outline of these views in the Lancet in 1856.

After making some further general observations to prove the strength of his argument, Dr. Tyler Smith proceeded to say,—"In conclusion, I may observe that it seems to me the great use of the knowledge of the mode in which retroversion of the gravid uterus occurs will be in the prevention of the full retroversion, or strangulation, as I have ventured to term it, of the gravid uterus in the pelvis. As long as retroversion was supposed to take place suddenly and mysteriously, little could be done to avert it; but if, as I believe, the displacement dates from the very beginning of pregnancy, in the great majority of cases, we may do much by position, and attention to the bowels and bladder, to prevent any dangerous symptoms; and, aware of the condition of the uterus beforehand, we shall be more ready to give prompt

mechanical assistance when it becomes necessary to pass the hand into the vagina to carry the fundus above the brim.

When retroversion has existed in early pregnancy, but has been relieved spontaneously or otherwise by the ascent of the fundus, labour takes place without any unusual difficulty. We ought, however, in the management of the puerperal state, to endeavour to prevent a return of the uterine displacement. The occurrence of pregnancy is rather favourable than otherwise to the cure of retroversion. In the latter months of pregnancy, the fœtus acts as an intra-uterine pessary; the organ is straightened, and in the return of the uterus to the size of the unimpregnated state by the process of involution, we have a better chance of curing retroversion than under any other circumstances. The abdominal bandage should not be tight enough to force the uterus into the pelvis. The patient should be encouraged to lie on her right or left side, inclining to the prone position, but avoiding recumbency. The bladder should be frequently relieved, and any violent straining during defecation avoided. She should remain in bed or on a couch longer than usual, and before resuming her ordinary duties the condition of the uterus should be ascertained; and if any tendency to a return of retroversion exist, an air-pessary should be worn in the vagina as long as may be necessary to ensure a right position to the uterus.

[We think that most writers on this subject are a little mistaken. It seems not to be sufficiently remembered that in nearly all women who have borne children, the womb has descended more or less from the position which it originally occupied in the unimpregnated state. It originally occupied the *axis of the brim*, *i. e.*, the os points downwards and backwards: as the parts become enlarged by pregnancy, it sometimes occupies the *axis of the outlet*, *i. e.*, the os points more or less downwards and forwards, in the first two or three months of pregnancy. But where is the fundus situated in this last position? Generally on the rectum more or less, in fact it is in a state of retroversion. We think that retroversion of this description is far more common than is generally acknowledged, and is constantly mistaken for the retroversion of Hunter and modern writers. If you introduce the finger and press it upwards at the back of the cervix you will feel the womb like a tumour. If you introduce the finger of the left hand into the rectum, and the finger of the right hand into the vagina, you may compress the womb between the two fingers: but this is so common that we cannot look upon it as of much consequence, although we think that it is often called retroversion, and is even mistaken for hypertrophy of the posterior portion of the womb. Now, most of what Dr. Tyler Smith says in the above paper may more or less be explained by the views which we have just expressed about this common malposition, and we agree with Dr. Oldham, who in another part of this paper, which we have not abstracted, says, that these cases do not give much trouble if not meddled with too much.]—*Lancet, Nov.* 17, 1860, *p.* 489.

91.—ON REDUCTION OF RETROVERSION OF THE UTERUS.

By Dr. Thomas Skinner, Physician to the Northern Dispensary, Liverpool.

[Dr. Skinner states that the following method of reduction is nearly painless, and most effectual in its results. He says:]

Let me premise that if there is any pain or difficulty likely to be experienced, the induction of anæsthesia will greatly facilitate matters; and whether the anal and vaginal orifices are rigid or not, the patient should previously sit over the steam of hot water, or be steamed in bed by the application of flannels, wrung out of boiling water, to the perineum. The preliminary steps having been taken, the bladder and bowels having been emptied, and the bearing-down efforts stopped, the patient should be placed upon her left side, with the pelvis well raised, the shoulders low, and the thighs as much flexed upon the abdomen as possible. If the cervix is bent upon itself, or if it is resting upon the arch of the pubes, it should be gently yet steadily pressed backwards and towards the left acetabulum or the left ileo-pectineal ridge, so that it may correspond as much as possible to one extremity of the right oblique diameter of the brim. Then insinuate the fore and middle fingers of the right hand, well lubricated with oil or lard, into the rectum, directing them towards the left side of the displaced uterus and of the pelvic cavity; exert a well-sustained upward pressure with them in the direction of the space between the right sacro-iliac synchondrosis and the pectineal eminence, and the tumour will most likely pass slowly from the true pelvis into the abdomen. If the patient is not under the influence of chloroform, the pressure of the taxis should only be exerted during expiration, which ought to be prolonged by the patient. At one time I thought it was necessary to introduce a ball-pessary, or some such support into the vagina, in order to prevent a recurrence of the retroversion; but I am now quite convinced that if the reduction is complete, and it is done subsequent to the second month, any kind of artificial support is much more likely to do harm than good. The patient should simply be ordered to take a little more than usual rest, to attend to keeping the bowels open with cold enemata once a-day or every other day, and to empty the bladder without straining whenever she feels the slightest inclination to do so.

If the above method of applying the taxis were to fail, I would not be disposed to invert the patient by placing her resting on her thighs and knees in the bed, and on her elbows or hands on the floor, as has been frequently attempted with indifferent success by many; the attitude is painfully tiresome to the patient, most repulsive to her feelings of delicacy, and besides there is nothing to be gained by the position, as that which I have described secures all that can be desired, —rest, comfort, freedom from unnecessary exposure, relaxation of the abdominal and femoral muscles, and the aid of gravitation.

The following method is so simple and ingenious, so safe, and easy of application, that I shall first allude to it. I mean the inflation of the vagina with air, as practised by Mr. Halpin, of Cavan, in 1839, and first made public by him in the Dublin Journal for 1840. Mr. Halpin fixed a recent bladder to the tube of a stomach-pump fitted with an air-tight piston, (there is nothing better suited to the purpose than the caoutchouc barrel syringe of Mr. Higginson of this town) and introduced the bladder empty into the vagina. Mr. Halpin retained it there by making a firm counter-pressure with his hand over the vulvar orifice, whilst his friend Dr. Finlay of Belturbet forced air into the bladder by slow degrees, until the fundus was reduced, and the os uteri was felt in its proper place; thereby affording additional proof of the view I advocate, that the cervix ought to be second in the order of reduction, and that it will right itself if the fundus alone is attended to. Although some of us may be disposed to question the practicability of such a method, it is no less a great fact that Mr. Halpin, assisted by his friend Dr. Finlay, did succeed in reducing with comparatively little pain and with no difficulty, a retroverted gravid uterus at the fourth month, which had previously resisted on various occasions, the use of aperients, hip-baths, leeches, catheterism, and the most approved modes of applying the taxis.

In September 1855, unaware of Mr. Halpin's method by inflation, I adopted the following plan. The case was the same as that in which I used the sponge tents in order to gain the *meatus urinarius*. I inserted pieces of compressed sponge into the vagina and rectum, placing them as much as possible towards the left side of the pelvic cavity, resting as it were upon the tuberosity of the left ischium and the greater and lesser ischiatic ligaments, in order to raise the fundus in the direction of the right sacro-iliac synchondrosis. Tepid water was injected, while fresh supplies of the compressed sponge were added as the fundus ascended, and every assistance was now and again given by a steady pressure with the fore and middle finger of the right hand; within half an hour the os was felt in its natural position. The patient was four months and one week advanced in pregnancy. As already stated, great difficulty was experienced in passing a catheter; the digital taxis, just described, was perseveringly tried in vain; ultimately, the compressed sponge and tepid water, combined with digital pressure, reduced the uterus with ease, and in safety both to the mother and fœtus, as the patient was delivered at the full time of a living infant.

As regards the preparation of the compressed sponge, any quantity of it can be got ready within an hour. Take one or two large common-place sponges; steep them in hot water, wring them, and roll or tie them close and tight with whip-cord or tape, so as to express as much of the moisture as possible, and place them in an oven to dry. When perfectly dry, unwind the cord, cut the compressed sponge with a sharp knife to the form and size desired, dip the point or edge of

insertion into a mixture of equal parts of bees' wax and lard or tallow in a state of fusion, and it is ready for use. By using compressed sponge and water as a mechanical lever, the pressure is equally, slowly, and steadily exerted on every point of the surface exposed to it, and on this and other accounts, it is much superior to passing the whole hand through a previously undilated vaginal or anal orifice.

Since reading the account of Mr. Halpin's excellent method by inflation, another plan has suggested itself to me; namely, to use hydraulic instead of pneumatic pressure, and two bladders, one for the vagina, and another for the rectum, instead of distending the vagina alone. For this purpose, the following arrangement is necessary: A metallic tube, three inches long, dividing into two branches, each about two inches in length, and Y-shaped; to each of the shorter extremities attach a recent sheep's bladder, and to the longer one, a short piece of India-rubber tubing, so that the whole may be attached to one of Higginson's or any other syringe; with a little tepid water, all is complete. (If any one is disposed to try bladders of caoutchouc, instead of ordinary bladders, he will be disappointed, because when two India-rubber bladders are filled at once from the same conduit tube, if there is the slightest variation in the thickness or elasticity of the material, the strongest cannot fill until the weaker is full and bursts, which is not the case with bladders obtained from animals.) Hydraulic pressure is every way superior to inflation with air; by the above method, both the rectum and vagina being filled at once, the tightened recto-vaginal septum will in no way interfere with the distension of the entire pelvic cavity, and the vagina will be relieved of a portion of the necessary distension of its walls; besides, inflation opposes one spherical surface containing air, to another containing water, which necessarily causes the uterus to bulge laterally, thereby increasing the impaction more or less; whilst a fluid wedge of water moulds itself to the surfaces of the uterus and pelvic cavity, and compresses them alike at every point.

Should these methods of applying the taxis fail, I think we are then justified in introducing the whole hand gradually into the vagina as recommended by Naegele, and successfully practised in many of the cases on record. From my own experience of pelvic manipulation, I should be more inclined to favour such a proceeding *per rectum*, because of the mechanical advantages. A cart can be much more easily raised at the extremity of the shaft than at any part of the body; so also with the retroverted gravid uterus, the further we apply our force from the centre of motion, the cervix, the more easy will be our efforts at reduction. Perhaps a combination of both, left to the discretion of the operator, will be found to be the proper course. As to the use of such auxiliaries as venesection, the warm bath, tartar emetic, and the like, they also must be left entirely to the good judgment of those in attendance.

There is a plan lately recommended by an associate, Mr. Greaves of

Manchester, to which I will allude; namely, that of exerting pressure on the fundus uteri with two fingers of the left hand *per rectum*, and with the palm of the right hand, making a *counterpressure above the pubes*. Mr. Greaves says, "by this manipulation, persisted in for a few minutes, I succeeded beyond my hopes." (Journal, 1859, p. 270.) From Mr. Greaves's interesting description of the case, it appears that the first stage of labour was nearly completed, that most probably a pain was on when he was engaged in applying the taxis, and that the membranes ruptured just before or about the time of the sudden reduction; that the patient was six months advanced in pregnancy, and that the pelvis "was more than average size." Under these circumstances, I quite agree with Mr. Greaves that, "if the method employed by him did no good, *the pressure of the open hand through the abdominal walls can scarcely do harm*." I would add that, if pressure is at all justifiable, it ought to be used simply as a support, or to press the os and cervix gently from the mesial line, so as to cause them to occupy the anterior extremity of the right oblique diameter of the brim, where they will obtain greater space and facilitate the reduction of the fundus. As regards this method, the term *counterpressure* should not be used, because I have already shown that counterpressure is contrary to the mechanism in two ways; first, it counteracts the efforts we are making to raise the fundus *per rectum*, and second, it is an attempt to reduce that which ought to be second in the order of reduction, the cervix, before or along with that which alone ought to be first in the same order, the fundus. Of this last mode of reduction, I would further remark, that the first who practised it was the father of the present Dr. Ramsbotham. He made use of it in 1817 in a case about the fifth month of pregnancy. (Practical Observations, vol. ii, p. 441.) Subsequently, in 1844, the same method was practised by Dr. De Billi, of Milan, in a case of his, eight and a half months pregnant. (Edinburgh Monthly Journal, 1845.) And lastly, it has been resuscitated by Mr. Greaves, who, it would appear, was not aware that it had been previously adopted. It is chiefly applicable in cases detected beyond the first half of the period of gestation.

Should a judicious perseverance in the use of all such means prove unsuccessful, we have only one alternative, namely, to produce abortion by tapping the liquor amnii. We may do so in one of two ways; first, by rupturing the membranes through the os uteri, by the finger, the use of a stylet or suchwise, if possible; and secondly, by the operation of *paracentesis uteri*, as first recommended by Hunter, and practised so very successfully by Mr. Baynham of Birmingham; only I would give a decided preference to the operation being performed *per vaginum* than *per rectum*, as a recto-vaginal fistula would be less likely to result, and the placenta would be less in danger of being wounded, which happened in two places in Mr. Baynham's case, which was operated on *per rectum*.

In conclusion, I would observe, that even in the simplest case, whatever method is adopted, there is danger to be apprehended, and less speed to be obtained through haste, whilst the exercise of gentleness, patience, and perseverance, affords us the only safe prospect of real and permanent success. I will only add, that in every case the retroversion ought on no account to be considered reduced until the os uteri is felt to occupy its normal position in the pelvis.

[We think that Dr. Skinner has not given due credit in this paper to the *position* which a patient ought to be placed in when about to have her retroverted womb restored to its proper place. Bear in mind that in these cases the fundus uteri is downwards and backwards, and ought to be upwards and forwards. What, then, is the best position in which to accomplish this restitution of position? Take a pelvis and examine the axes, hold a pear by its stalk and let it drop downwards and backwards into the hollow of the sacrum in the upright position, do the same when the pelvis is placed in the usual obstetrical position. Now try to rectify the pear by pushing it up with the finger or with an instrument and you will have great difficulty, but reverse the position of the pelvis, place it on the table *inverted* and resting on its pubes and promontory of the sacrum, and the pear will drop into its place almost by the force of gravitation. So in these cases we ought to place the woman with the pelvis on the edge of the bed, the legs across the bed in the prone position, with the head and face towards the floor. In this position gravitation will most materially assist any manipulation with the finger either in the vagina or rectum. Moreover, the position can be maintained for a considerable period, and facilitates rather than interferes with the operations of the finger in the rectum, and does not much hinder the same in the vagina. Dr. Skinner's advice about the hand in the rectum is objectionable, although we believe the hand *could* be introduced into this canal if all other methods failed.]—*Brit. Med. Journal, Dec.* 8, 1860, *p.* 949.

92.—ON THE INDUCTION OF PREMATURE LABOUR.

By Henry James, Esq., Surgeon and Accoucheur to the City of London Lying-in Hospital.

The important position which the induction of premature labour holds amongst obstetric operations, from the great interest which attaches to it as a means of saving the life of the child as well as of alleviating the suffering of the mother, has induced me to communicate to the profession the method which I have for many years adopted for its induction, with almost uniform success. And here I may add that I have learnt, since the time when I first, in 1848, used the operation to which I refer, that Dr. Stultz also successfully adopted a similar plan at an earlier date—namely, in 1834.

When about to perform the operation, the patient is placed in the

usual obstetric position, and the forefinger of the left hand is then passed up to the os uteri, and, as soon as possible, between its lips into the uterus; the neck is now to be slightly pulled down, and the finger, still within, is to be passed round it as far as possible. But the passage of the finger through the os uteri is seldom accomplished upon the first attempt; upon the second trial, however, the vagina will generally be found to be more lubricated, and the os also to be situated lower down; and frequently it will be necessary to repeat the manipulation upon the following days until a third or a fourth time; and great assistance may also be obtained by the patient bearing down, or by pressure being made with the right hand of the operator upon the fundus of the uterus, because in cases requiring such aid the uterus is kept high up by the unusual prominence of the promontory of the sacrum. Upon each of the above occasions some pains will usually ensue, and ultimately the sensations of labour will come on; when an elastic tube, or a male catheter (No. 10), free at one end and attached at the other to an elastic bottle, is to be passed up through the os and between the walls of the uterus and the membranes to the extent of from four to six inches. In doing this, considerable care is required so that rupture of the membranes may be avoided; and if any resistance is felt to the passage of the tube it should be withdrawn and passed in another direction. Mention is made of this more especially, because I once experienced such an obstruction, and upon pushing the catheter on, a stream of blood came through it; and I inferred that, without doubt, the catheter had been passed under the placenta. The catheter or tube having been in this manner inserted, pressure is to be made upon the elastic bottle connected with it, which has been previously filled with eight ounces of cold water, and in this way a sensation of cold is conveyed to the child, which generally causes it to plunge about; afterwards pains follow, and the water which has been injected is generally returned in gushes. So soon as the os is dilating and the membranes are at all protruding, they should be ruptured, and the birth will speedily take place. The patient should be desired to move about freely during the period, and a brisk purgative may also be given. I have made use of the secale as an auxiliary, but have been disappointed with the result.—*Lancet, March* 2, 1861, *p.* 210.

93.—*Annular Laceration of the Cervix Uteri.* By Dr. T. Herbert Barker.—The term "annular" was chosen to distinguish that form of laceration during labour by which a complete ring of uterine structure, consisting of the cervix and os uteri, is separated from the remainder of that organ, the child having passed through the laceration, and not through the os uteri. The cause of this kind of laceration is, the protracted pressure of the head against a circle of the uterine parietes in a contracted pelvis,—perhaps, also, conjointly with

a sharp linea ileo-pectinea. In the case related, the author was called; on the fourth morning of labour, to a primiparous patient, 42 years old, when he found the os uteri was not dilated larger than a florin, the pelvis contracted, and the bladder much distended. Soon afterwards, an ear could be felt under the pubes, through a transverse laceration in the cervix uteri, anteriorly. An attempt was made to deliver with the forceps, but unsuccessfully, and craniotomy was performed. After the birth of the child, a circular mass was found in the vagina, consisting of the os and cervix uteri, separated from the body of the uterus at every point, with the exception of an inch and a half posteriorly. The os uteri was of the size of a florin, and its margin thin, even, complete, and well defined. The lacerated edge was jagged, rough, rather thicker than the margin of the os, but not so thick as the intervening uterine texture. The colour of the entire mass was of a purplish-red, venous tinge. There was no hemorrhage. It sloughed away on the third day. The catheter was required for seven days afterwards. The patient recovered, and the catamenia returned. With regard to the treatment, Dr. Uvedale West, in a letter to the author, writes that in such a case he would endeavour to dilate the os uteri by introducing first one, then a second, then a third and a fourth finger, side by side, to procure sufficient dilatation to admit either the vectis or the forceps.—*Lancet, Dec.* 15, 1860, *p.* 586.

94.—*On Turning.* By JOHN PEARSON, Esq., Staleybridge.—[Mr. Figg in a paper lately published in the Medical Times and Gazette, attributed to Dr. Simpson the merit of having revived in Great Britain the operation of turning in cases of pelvic distortion. Mr. Pearson says:]

Much as I admire that eminent Physician-Accoucheur, and however grateful I feel for the very valuable contributions he has made to Obstetrical Science, I cannot allow such an assumption to pass unnoticed, as I am personally able to state that for fifty years past at least, that excellent method of terminating difficult labours, has often been practised in this town. When I joined the late Mr. Hutton of this place as assistant in the year 1823, he very early instructed me in this method of safely conducting difficult parturition, stating that in the course of his long practice, extending from the year 1800, he had often had recourse to it successfully. The most remarkable instance which I now recollect of the value of this operation, in which I was concerned, occurred in the year 1835, when I was called in consultation to a case of great pelvic distortion, formed by the sacrum projecting so much as seriously to diminish the conjugate diameter. This was a second labour, the female having been delivered of her first child two years before, by craniotomy, under the care of a very talented surgeon in Bolton. When I was called in she had been in labour about twelve hours, the os uteri was well dilated, but the head

had not made the slightest advance into the pelvis. I at once suggested to the medical attendant the possibility of effecting the delivery by turning, instead of again having recourse to craniotomy, which offered the only alternative. This gentleman declined to make the attempt, but consented to my undertaking the management of the case. I am glad to say, I succeeded in delivering a male child, asphyxiated at first, but which soon recovered, and is now a healthy man. Two years afterwards I delivered the same female of a second child by the same process, a female, and who is now a mother. This woman is still living.—*Med. Times and Gazette, Nov.* 10, 1860, *p.* 466.

95.—*Spurious Pregnancy.*—[The following is extracted from a lecture by Dr. Priestley, delivered at the Middlesex Hospital.]

Perhaps the most curious of all the imitative diseases are those conditions named respectively hysteric tympanitis and spurious pregnancy. These affections have often been mistaken for ovarian or uterine tumours, or for genuine pregnancy. The abdomen is projected forward, as if it contained a solid tumour or gravid uterus; and if there is much fat in the subcutaneous tissue, the muscles of the abdomen, being unusually tense, yield a sensation to the hand very similar to that experienced when a solid mass is really present. In cases of this kind, the abdomen has in fact been tapped, and on several occasions been laid open by incision, for the purpose of excising an ovarian tumour, when none was present. The use of percussion ought, of course, in these cases, to do much in clearing up doubts concerning the nature of the abdominal swelling; but more conclusive still is the evidence afforded when we administer chloroform to the patient. If the case be one of hysteric tympanitis or spurious pregnancy, uncomplicated with solid tumour, the swelling entirely disappears when deep anæsthesia is induced, but it reappears as consciousness returns.—*Brit. Med. Journal, Feb.* 23, 1861, *p.* 190.

96.—A NEW CRANIAL PERFORATOR.

By Dr. GAILLARD THOMAS, Secretary to the New York Academy of Medicine.

[There are some difficulties in the use of Smellie's instrument, (the one generally used), which Dr. Thomas thinks will be avoided by the employment of that invented by him.]

The instrument consists of a steel or iron tube, ten inches long, which ends in a screw, and hides within itself a cutting blade which is thrown out of its bed by the hand of the operator compressing the handles. Two inches from the extremity a shoulder is placed, which prevents its entering the head too far, and the blade is fixed upon its pivot by a slot, which renders its removal easy for cleansing the

instrument. It is hoped that this instrument will possess these advantages:

1st. There is no difficulty of introduction, for the screw, being a double one, catches readily in the scalp and penetrates the skull with the same ease, and by the same force which would cause a gimlet to enter wood, or the screw of a ramrod to engage the wadding of a gun.

2nd. It will not slip, for it is not pressed against the skull at all, but is wormed in by the process of boring.

3rd. The operator himself throwing out the blade, he can regulate its progress and thus prevent its cutting the surrounding parts; an advantage, however, possessed by Naegele's instrument.

4th. There is only one cutting edge, and consequently, this is easily guarded; the attention of the operator not being distracted by one on the other side acting in a different direction. Two or three essays have convinced me that the instrument will penetrate the skull with great facility and certainty, and safety for the mother.

In presenting it to the Academy, I wish to be distinctly understood as in no wise entering a plea for a more frequent resort to craniotomy, an operation which must diminish in frequency of performance *pari passu* with the advance of the obstetric art. But if, (as no one will deny) this sad operation must in some cases be done, it were better that it were well done.—*American Med. Monthly, Dec.* 1860, *p.* 429.

97.—CASE OF CÆSAREAN SECTION.

By Dr. James Edmunds.

[The patient was tall and well-formed, and thirty-eight years of age. She had had two dead children, the last six years ago, She had had pain in coïtu and fœtid sanguinolent discharge since this last confinement. Labour commenced of the third child on Dec. 4th. On the 6th,]

I was requested to see her, and found projecting into the vagina a mass about two inches in diameter, hard, tuberculated, and superficially ulcerated. I had great difficulty in making out the os uteri; but with

firmness and care I got the left index-finger through the mass, and managed with its tip to feel the head, my finger being then immersed over two inches in a dense, unyielding tissue, referable only to cancerous enlargement of the os and cervix uteri. Pulse about 90; pains frequent; muco-sanguineous discharge, and soft, warm, dilatable vagina. Nourishing diet, opium, and patience were prescribed. Afterwards I saw her daily: the pains continued; her pulse increased in frequency; she became much wearied, and on Sunday exhaustion was coming on, but the os had not yielded. She was a calm, intelligent woman, and I now explained the cause of her suffering and danger, gradually breaking to her the alternatives of her position—1st, exhaustion and death from obstructed labour; 2ndly, laceration of the os uteri, and probable death from arterial hemorrhage; or, on the other hand, two practicable operations for her relief—1st, slitting up the os uteri, and delivery by the forceps or embryotomy; 2ndly, *the Cæsarean section.*

I made her fully understand each procedure, with its risk and contingencies. I told her that the first would cut through a mass of diseased tissue, immensely supplied with bloodvessels, whose mouths would be out of reach, and would bleed so largely that both herself and child would probably perish; that I thought the latter, by direct incision into the bowels, though so frightful to contemplate, was the less dangerous. I left her to consider the matter, to consult her husband and parents, and then send them round to me, when I would further explain to them, hear their decision, and arrange to meet any other medical men they might name.

Her friends came—said that she had fully made up her mind, and if I would do what I thought best in the case, that, whether she died or recovered, they also should be satisfied. I thought it expedient for the responsibility of the case to be shared by others, and Dr. Giles, of Limehouse, saw her with me at midnight.

She had now been nearly six days in labour; there was considerable discharge; the vagina was soft and relaxed; her pulse, when recumbent, was 120, sharp, and weak; and her face showed anxiety and impending exhaustion; but there was the same hard, unyielding os uteri, and careful examination showed that there was no part healthy or dilatable. I had seen two thousand cases of labour, and my mind had decided for abdominal hysterotomy. Dr. Giles concurred without hesitation, and the operation was fixed for three a.m.

We left her, and my assistant, Mr. W. P. Dukes, administered a terebinthinate enema. Three other medical friends then met us at my house, and I read through a list of instruments and programme of procedure, which was discussed and adopted, and the operation then carefully rehearsed.

I ascertained that no one present had lately been dissecting, or was likely to be a source of infection; the instruments, needles, and silk were new and in perfect order; lastly, we all cleansed our hands most

thoroughly, and made our way into the bedroom in detachments shortly before the appointed time.

Mr. Edmunds, senior, Dr. Giles, Mr. Nightingall, Mr. Nelham, Dr. Meeres, Mr. Dukes, and myself were now present; and as the friends had been unable to engage a first-rate nurse, Mrs. James Edmunds had consented to be present, and take charge of sundry minor arrangements.

I now requested the four eldest to examine her most carefully; to wedge the finger through the cancerous mass, and reach the head if possible, to satisfy themselves that no fissure or dilatable part existed; and, in short, to finally decide upon this formidable operation, and share with me the responsibility of the proceeding, whatever its result. Unanimous concurrence followed, and the arrangements proceeded.

I had heard the brisk tic-tac of the fœtal heart in the left iliac fossa, where the child's back and shoulders proved to be, but could not satisfy myself of the position of the placenta. Dr. Meeres thought he heard it in the right iliac fossa, and Dr. Giles corroborated him; but I pointed out that a similar sound was also audible on the left side, and we then thought both proceeded from the respective iliacs sounding through the uterus; and as nothing like the souffle was disovered anywhere else, and through the os the head was distinctly felt to be covered by the membranes only, we conjectured that it was seated at the back or fundus, and was placed well for the operation. In fact, however, it was attached to the front of the cervix, extending across, and simulating the sounds often heard from the iliacs when the patient is recumbent. She had passed urine just before our arrival; nevertheless, I used a male elastic catheter, and drew off a teacupful of urine; then with a large silver catheter I broke through the membranes, and let out some ounces of liquor amnii.

The patient, having a warm flannel petticoat loosely tied round her waist, was now seated on the edge of the foot of the bed, her legs hanging over, and the head and shoulders resting comfortably on pillows. Mrs. Edmunds was superintending the water, basins, bath, and other fire-place arrangements; Mr. Dukes had charge of the instruments, ligatures, &c.; Mr. Nightingall and Mr. Nelham each took one side of the abdomen, to keep its wall in close contact with the uterus while the incisions were being made, so as to prevent blood or fluid from flowing into the cavity of the peritoneum, and to hold back the intestines when needful; Dr. Meeres had charge of the chloroform and I was to use the knife.

I now most thoroughly cleansed my hands, to free them from urine, or other possible contamination. The chloroform was commenced, and in a few minutes the patient was insensible. I drew her flannel down to the hips, and with a large heavy scalpel made an incision upon the inner edge of the rectus muscle, and cut into its fibres from

the navel down to near the pubis where I deepened it cautiously on account of the bladder.

I then opened the cavity of the abdomen at the middle of the incision, carefully completed it downwards, and then, making a director of my two left forefingers, I rapidly slit up the peritoneum, the knife's back being to the intestines, and its point preceded and carefully guarded by the fingers. The opening exposed the uterus, of a purplish-rosy colour, everywhere smooth and glistening, except at the top of the incision. Here a roundish projection appeared, resembling a cancerous nodule: but it yielded readily to pressure, and was, in fact, a space where the muscular tissue was absent—a sort of uterine hernia. In horses which have been staked sufficiently to break the muscular wall, but not to lay open the cavity of the abdomen, analogous bulgings are often seen. Doubtless, unexpected uterine ruptures often spring from such imperfections of structure.

I now cut into the uterus from above downwards in the median line, and its fibres retracted; but, on deepening the incision, blood gushed rapidly out. I involuntarily said, "We have the placenta," and some one else said, "She is dying;" but I instantaneously plunged the knife completely through the uterus and placenta, cutting upwards as I withdrew it. I then dropped the knife from my right hand, thrust the two left forefingers through the wound, and hooked out a portion of the placenta, drawing it carefully through, and seizing it again with the right and left hands alternately until I had extracted the whole, when I dropped it, and recovering the knife again, used the left fingers as a director whilst I slit up the uterus, as I had previously done the peritoneum.

The child was in the first position, and, relinquishing the knife, I seized the feet, and, steadying the uterus with my left hand above the incision, carefully withdrew the child and passed it to Mr. Dukes, who made it cry, and separated the placenta.

As the child was drawn out the uterus embraced the head; and when this escaped, it further contracted and immediately receded, the intestines at the same moment flowing out from above in a most troublesome manner. A piece of membrane showed through the uterine gap, I tried to drag it out, but it broke away, and feeling that the membranes were adherent, I left them, but carefully pushed back into the uterus what shreds I could not tear away. The uterine wound now seemed about two inches long, only slightly gaping, of a pale flesh colour, and free from blood.

Mr. Nightingall and Mr. Nelham, on each side, now sustained the intestines most judiciously; whilst I, using my left hand to back in and guard the viscera, put in the first stitch less than an inch from the top of the incision, including no muscular fibres, only skin and adipose tissue, Mr. Nelham seizing the needle and drawing it through, whilst my hands held the intestines and skin in place. In this way the wound was closed with ten stitches of the interrupted suture—the

first six of double silk, and four single threads at the intervals. The needles were three inches long, and moderately curved; the silk as large as small whipcord, and well waxed.

The abdomen being sponged, the edges were accurately adjusted, and secured with strips of non-resinous adhesive plaster on unglazed calico; then a firm roll of lint, the size of a cane, was held on each side of the incision, whilst I fixed them with plaster, so as to press together the deep parts; and afterwards a long four-inch bandage was rolled round the abdomen.

The pillows having been shifted, she was drawn up into a nearly horizontal posture, and covered with a woollen shawl, so as to be cool, yet free from draught; more clothing was to be added when she felt chilly. A drachm of nepenthe, in a little brandy-and-water, was now given to her, absolute repose enjoined, and she was to drink from a teapot beef-tea, broth, and milk-and-water, at her own discretion.

After careful examination, we estimated the entire loss of blood at about fifteen ounces.

She afterwards took mild nourishment, with fruit, and occasionally a little wine-and-water.

The after-pains were relieved by a few doses of anodyne, and the catheter was sometimes required.

On the fourth day, there was general peritonitis and constant vomiting, but the breasts filled. Ice, and nothing else, was taken by the mouth; beef-tea and anodyne per anum; and on the next day her pulse was down to 120, the sickness passing off, and the bowels responding to the presence of the long tube. At night, the plaster was softened and removed, the six principal stitches taken out, and the wound secured with plaster and bandage. The other stitches were removed on the eighth day. On the eleventh day, the wound was beautifully united; and on the fourteenth it was so firmly consolidated that coughing hurt her but little.

After the sixth day, the peritonitis gradually subsided, and the bowels and bladder acted naturally; milk continued to run from the breasts, but no lochia appeared till the ninth day, after which they were abundant. On the tenth day, she was on the sofa to her dinner; on the twelfth, she sat up for a few hours; and on the fourteenth day was convalescent, and said she had got on as well as after her former confinements.

She has since gained strength rapidly, is now suckling the baby, and doing marvellously well.

Remarks.—Irrespective of the actually successful results, we believed that Cæsarean section gave this mother a better chance than embryotomy. Therefore we incurred not the painful task of weighing the infant life against increase of maternal risk.

The position of the placenta was a curious coincidence, and had we known it, would perhaps have deterred us from the operation; yet this case will establish the fact that, if it be done with resolution and

rapidity, the placenta may be cut through and dragged out first, without losing either mother or child, and also that it is unnecessary to endanger the arteries on either side of the uterus in order to avoid it. The gush of blood which followed the second stroke of the knife left no time for consultation or even for thought, and I instinctively adapted Dr. Simpson's practice to the emergency. The placenta was transfixed a little above and to the left of its centre.

The remaining of the membranes caused much anxiety, still they seemed firmly adherent, and it was best to leave them. I think it most important that the protruding shreds were pushed carefully back into the uterus, else the peritonitis would probably have been fatal. No membranes have passed per vaginam. The lochia were delayed, though the breasts filled on the fourth day, and the milk remaining through the after symptoms sustained my hopes of her recovery. The turpentine was used to clear out the gaseous and feculent contents of the intestines, and to prevent hemorrhage. The exclusion of all sources of infection, the purity of the instruments and sponges, and thorough cleansing of the hands, are points to which great care was given. Excepting a simple anodyne, no drugs were administered; no mercury on the one hand, and no brandy on the other. She had simple and varied nourishment with fruit.

I thought these great incisions more likely to be healed by nature in her own way than by nature under the influence of either mercury or brandy. The peritonitis, though most severe, was treated simply, and the result will confirm me in the practice.

This case will go towards diminishing the hitherto frightful death-rate of our Cæsarean operations.—*Lancet, Jan.* 5, 1861, *p.* 4.

98.—ON A NEW SIGN OF POST-PARTUM DETACHMENT OF THE PLACENTA.

By J. Clay, Esq., Senior Professor of Midwifery, Queen's College, and Surgeon-Accoucheur to Queen's Hospital, Birmingham.

[The rules usually given in obstetric manuals for the management of the placenta, after the birth of the child, are:—to wait for a pain, or, to feel for the insertion of the cord into the placenta. But both of these may mislead, and the frequent examination necessary annoys and pains the patient.]

Four years ago I was led, from these causes, to investigate the subject, with the view of improving, if possible, upon the old mode of managing the deliverance of the after-birth. I thus ascertained certain facts, from which I came to the conclusion, that a very simple sign existed by which the separation of the placenta, after the birth of the child, might be indicated; and, having tested it in upwards of nine hundred cases, I hope I may be considered to be fairly entitled to lay the results at which I have arrived before the profession.

Before dividing the umbilical cord, I always apply two ligatures, and make both sufficiently tight to prevent the occurrence of hemorrhage. If the maternal part of the cord is now examined, it will be found to be in a flaccid condition, and almost free from blood; but if it be again examined, at an interval, say from one to three minutes, it will be found to have acquired increased specific weight, and that the vessels are more or less filled with blood. The one fact may be ascertained by poising the cord on the fingers; the other by slightly grasping the cord near the vagina, with the thumb and fore-finger of the left hand, and, with the fingers of the right hand, suddenly compressing it, when a well-marked sense of fluctuation is perceived underneath the fingers of the left hand,—a kind of resiliance similar to the feeling produced when an elastic tube filled with fluid is suddenly compressed.

When the placenta is thrown off, or sufficiently detached to give rise to a tolerably free hemorrhage, the cord loses its increased specific weight and the hydrostatic property just mentioned. These phenomena occur so invariably, that *the loss of the previously acquired hydrostatic properties of the cord after the birth of the child constitutes the sign of detachment* previously referred to.

The whole of the phenomena are manifested in three stages, viz.:—1st, a state of flaccidity; 2nd, a state of repletion; 3rd, a state of flaccidity.

If the umbilical cord be tightly grasped by a spasmodic contraction of the os uteri, or by irregular contractions of the body of this organ, the *loss* of the particular hydrostatic properties may be delayed for a brief interval; but in a few seconds the spasm subsides, and those phenomena are produced which indicate the separation of the placenta, and that this structure may be safely extracted. These signs are not, of course, always equally marked in every case,—often requiring experienced tactile management, in order to detect their presence. When the uterus is in a flaccid condition, the phenomena are manifested in a very slight degree, but are still perfectly reliable. On the other hand, when the uterus is contracted, with some degree of firmness, on the placenta, they are so well marked that the most inexperienced may readily detect them. In cases of partially adherent placenta, the disappearance of the hydrostatic properties, after being once fully developed, and the failure to deliver the placenta by the usual manipulations, have always indicated to me the necessity for promptly adopting artificial detachment by the introduction of the hand. In twin cases, if the cord is firmly tied, I have invariably found that the signs persisted until the birth of the second child. In one case, where the hydrostatic properties disappeared, after being well marked, before the birth of the second child, I found, on examination, that the corresponding placenta was detached, and I at once removed it, which otherwise would probably have been suffered to remain. Neither mother or child incurred any risk.

It sometimes occurs that the placenta is separated simultaneously with the birth of the child. In this instance, the first series of phenomena may be absent; and it may be prudent to wait before proceeding to extract the placenta, although it may be generally effected with safety.

The practical value of the application of these facts to obstetrics is obvious, as by merely compressing the cord in the manner previously indicated, the precise time of separation may be easily ascertained, the placenta at once extracted, and the patients thus freed from those frequent annoying examinations usually employed. The prompt delivery of the placenta, on the first efforts of the uterus, is very important, as this organ contracts then more efficiently, and the risk of hemorrhage is not so great, and it may be fairly assumed that the puerperal convalescence is not so protracted as under a more dilatory proceeding.

To students, or inexperienced practitioners, it might be a safe instruction to impart,—not to interfere in the extraction of the placenta so long as the hydrostatic properties herein defined are persistent.—*Dublin Quarterly Journal, Nov.* 1860, *p.* 372.

99.—ON SUB-INVOLUTION OF THE UTERUS AFTER DELIVERY.

By Dr. J. Y. Simpson, F.R.S.E., Professor of Medicine and Midwifery in the University of Edinburgh.

[The patient, whose case forms the text of this lecture, had suffered from disordered menstruation, and a feeling of weight and discomfort in the pelvis, since the birth of her last child. By examination it was found that the uterus was equably enlarged to about the size of a uterus in the third month of pregnancy, and readily admitted a sound to the extent of three and a half or four inches into its interior. The patient was suffering from an arrest in the retrograde metamorphosis that normally occurs in the uterus after parturition.]

After parturition, the greatly enlarged uterus begins regularly to involve or absorb, and rapidly diminishes in size, until it has been reduced almost to its pristine dimensions. I have had occasion, in another department of our course, to call your attention to the wonderful changes that go on in the uterus in consequence of impregnation, and to tell you how, from being a small body, two or three inches long, it comes, in the short space of nine months, to attain the length of a foot or more, while it is correspondingly enlarged in all its other dimensions and diameters. And I may here repeat the conviction that I then expressed, that if ever the laws of nutrition are to be clearly made out, the inquiry will probably be most successfully pursued by investigating these laws as they are seen in operation on a large and gigantic scale in this very organ; for in no other organ, so

far as I remember, do we ever see nutrition and growth going on so rapidly as here, where out of a mere mass of nucleated fibres and cells an enormous body of numerous and well-marked muscular fibres become developed within the course of the nine months of pregnancy. Hardly less wonderful than this great development of the uterine walls during the progress of gestation, is the still more sudden diminution that occurs in these walls after parturition has been completed. The muscular fibres—perhaps weakened and exhausted as a result of their violent action during the parturient process, and so rendered prone to degenerate—and deprived also to some degree, of the supply of blood brought to them so profusely during the time that the uterine circulation was so much exaggerated, now undergo, after parturition, a fatty metamorphosis, in consequence of which they almost all melt down and disappear; so that in the brief space of five or six weeks the whole organ dwindles down, and diminishes to nearly its original dimensions. The muscular walls of the uterus are not absorbed as muscle, but, like many other effete structures, they first undergo fatty degeneration, and are absorbed as fat. This fatty degeneration commences on the inner layers of the walls, and passes from them to the outer layers. The fatty metamorphosis of the uterine muscular fibres appears under the microscope, a few days after delivery, as a series of glistening particles deposited in the course of each individual fibre. Now the patients, with whose cases I introduced my present observations, are suffering from this curious condition of the uterus, that after the birth of their last children, this retrograde metamorphosis of the uterus has not taken place during the puerperal month, or, rather let me say, has taken place only to such an imperfect degree that the uterus is of the size we usually see it have at the end of the first week or so after delivery. They are suffering, then, from a hypertrophy of the uterus, which is pathological in its permanency, but which results from a hypertrophy purely physiological in its origin.

Its Ætiology.—What, you will be ready to ask, is the cause of this enlargement of the uterus remaining permanently? How is it that the retrograde changes in the uterus become arrested and the absorption or involution of its walls is prevented? To these questions it would be difficult, or indeed impossible, with our present limited knowledge of the processes of development and degeneration that go on in the uterus, to furnish a satisfactory reply; but let me point out to you some causes which occasionally appear to lead to the production of this hypertrophy from arrested involution of the uterus.

1. *Rising too soon after Confinement.*—We know that both in the healthy and in the morbid state the uterus is apt to become more congested when the patient assumes the erect position, and that a morbid degree of congestion interferes with various physiological functions. And we can easily imagine that if a delicate woman gets up too soon after her delivery, and remains for a lengthened period erect, while the womb is still more than usually large, the circulation

in its walls might get so impeded, and such an amount of congestion be produced, as would prevent the normal changes in its walls, and impede the free absorption of its disintegrated particles.

2. *Repeated Miscarriages.*—However it may be, we know, further, that in a number of cases an enlarged condition of the uterus of the same nature as that of which I am speaking, results from the frequent and rapid recurrence of a series of miscarriages and abortions in the same patient. You are aware that a woman who has once aborted is extremely liable again to abort at the same period whenever she again becomes pregnant. In such cases it is, moreover, remarkable that the patient often conceives again in a very short time after the occurrence of the abortion; and when this process has been several times repeated—the uterus undergoing the enlargement of a new pregnancy, before it has had time, as it were, to recover perfectly from the hypertrophy of a preceding one, the case sometimes ends in a complete disturbance of the normal physiological process of degeneration and diminution in the uterine walls; and the womb is left in a permanently hypertrophied condition.

3. *Metritis.*—Again, if you inquire minutely into the history of patients affected with this form of disease, and ask them particularly as to whether anything has gone wrong with them in their puerperal state, you will very frequently find, as in the case of the second of our Hospital patients, that within a short time after their confinement they have been the subject of an attack of inflammation in the uterus or ovaries, or neighbouring pelvic organs. They will, perhaps, tell you that a week or two after their child was born they had a shivering, followed by pain in the region of the womb and more or less fever, which compelled them to remain in bed for some time. It would appear as if the occurrence of metritis, or perimetritis, in the puerperal female, exerted such an influence on the substance of the uterus as to prevent the occurrence of those changes that lead normally to its diminution in size.

Its Semeiology.—The patient whom I have imagined you to be questioning as to the history of her malady, will probably tell you, further, that, after she had recovered so far from her inflammatory attack, and from the consequences of the bleeding and blistering to which she had been subjected for its cure, as to be able to walk about again, she began to be conscious of a feeling of discomfort in the lower part of the abdomen, to which she had never before been accustomed. There is frequently a sense of weight or bearing down of the uterus, of distress in the lower bowel, and of uneasiness in connection with the action of the bladder, and a weakness, sometimes amounting to actual pain, in the lower part of the back, and, in some cases, a numbness of the lower limbs, which are all new to the patient. She attributes, most likely, these feelings to weakness, and expects that as she gains more strength the uneasy sensations will disappear. But some months may elapse, and she begins to be disappointed at her never

getting entirely relieved of this local trouble; and after lactation is accomplished, if, indeed, she have been able to nurse her child at all, she finds that her menses do not return regularly, or become too profuse and painful. Leucorrhœa, too, is sometimes present to a greater or less extent; and altogether the patient is in a state of confirmed and anomalous bad health that is difficult of endurance. By means, perchance, of tonics and sedatives she strives to regain her health and alleviate her uneasiness, and continues for a time to hope that her former strength will still return, and that her ailments will disappear. Gradually, however, the conviction becomes forced upon her that something besides time will be needful for her cure, and that some kind of medical treatment is required for her relief. Accordingly she comes to you, and when you have heard a history such as that I have endeavoured to narrate, you come to the conclusion that she is labouring under some disease of the uterus. But what the precise nature of that disease is you will not be able to fix and determine by studying the mere history of the symptoms. To make out a correct differential diagnosis, you must institute an examination of the uterus.

Its Physical Diagnosis.—On placing the hand over the abdomen, you can usually feel the enlarged uterus rising in the form of a tumour out of the pelvic cavity, and lying above the pubes, more especially in those cases where the hypertrophy is very considerable. In all cases you can discover, on making an examination per vaginam, that the vaginal portion of the cervix uteri is enlarged—for this is a form of hypertrophy in which every part of the organ is implicated; and you find the whole uterus to be unusually large and heavy. This kind of local examination is, in the majority of cases, most satisfactorily carried out when the patient is laid on her back; sometimes it is more convenient to have her placed somewhat laterally. But in every instance of this kind you must bear in mind the importance of making an examination with *both* hands simultaneously,—the forefinger of one hand being employed to explore the uterus through the vagina, while the fingers of the other are applied to the fundus through the medium of the anterior abdominal wall. There is, perhaps, no variety of uterine disease in the diagnosis of which this sort of examination can be employed to more purpose than in the case of simple enlargement of the organ. In a few patients the abdominal walls are too thick to admit of your feeling anything very distinctly on palpation; and there are others, still fewer in number, in whom a certain degree of uneasiness renders the necessary pressure painful to the patient. When you have thus got the uterus between the two hands, you can easily recognise the existence of the hypertrophy, and even determine the degree to which it has taken place. You can feel that the swelling is not due to the presence of fibroid masses in the walls of the uterus, for there is no irregularity and no peculiarity in its shape. It has unmistakeably the shape and contour of the healthy organ; it is only that organ in a state of equable hypertrophy. Such

a condition of matters might still, however, possibly be due to the presence of a fibroid tumour growing from the submucous layers of the uterine wall, and projecting into its interior; and to make sure that there is no such tumour present, you must have recourse to another simple means of exploration, viz., the introduction of a uterine sound. The cavity of the healthy uterus measures usually, as you know, about two and a-half inches in length; and when the sound is introduced, you find its point is arrested at the fundus, when the knob placed on the convex side of the instrument, at two and a-half inches from the extremity, has reached the level of the external orifice of the uterus. Where the organ is enlarged, however, the knob I speak of slips past the guiding finger, and the instrument runs up into the interior to a depth of three or more inches; and when the enlargement of the uterus is of the kind I have been describing as dependent on defective involution of the organ after delivery or miscarriage, the instrument usually slips in at once without the slightest difficulty, for the uterine orifice and canal is preternaturally patent. When, on the other hand, the enlargement is due to the presence of a tumour in the cavity of the uterus, the point of the exploring instrument usually meets an obstruction immediately on traversing the canal of the cervix, and it may require, in such a case, the exercise of some skill and a little gentle manipulation in order to pass it into the interior of the uterus at all. Besides, after the sound has been fully introduced, you can often feel with it the projecting body; while in the case of the simply hypertrophied organ the sound passes freely and unobstructedly round in all directions. If with the sound thus introduced into its interior you raise the uterus towards the hand placed over the abdomen, you will be able to make out more distinctly and definitely than ever the real nature of the case. There is one difficulty in connexion with the use of the sound in such cases, of which I ought, perhaps, to warn you, and it is this: The great weight of the fundus of the uterus, sometimes aided by inflammatory adhesions, gives a strong tendency to various displacements of the organ, and more particularly to retroflexion of it. Such a complication you must, accordingly, be prepared to expect; and in passing the sound you must then remember to turn it with the point and concavity looking backwards towards the sacral promontory. It was not long after I had begun to make use of the sound in the diagnosis of uterine diseases and disorders, that I first fell in with and recognised a case of sub-involution of the uterus. The patient, whom I saw in consultation along with the late Dr. Abercrombie, was the wife of a medical gentleman, and there was much perplexity as to the nature of her disease. There was a large, rounded tumour laying over into the right iliac region, which was thought to be possibly either an inflamed ovarian tumour, or an abscess, as there was considerable pain on pressure. On passing a sound, however, it was found to run right up at once about four inches into the very centre and top of the tumour. The supposed tumour

was thus shown to be neither more nor less than the uterus considerably enlarged and turned somewhat to one side, as sometimes happens in such cases. That was the first occasion, so far as I know, in which this kind of uterine hypertrophy was clearly made out; but since then I have seen it very frequently, usually as a result of inflammation after delivery, but sometimes associated with repeated miscarriages in the relation both of cause and effect.

Its duration and degree.—In some cases you will detect the form of uterine hypertrophy which I am describing, within two or three weeks, or two or three months after delivery. These may be called the acute, or subacute, types of the disease. But oftener perhaps in practice, you will not be applied to till the disease is chronic,—till many months, or even years, have elapsed since the date of the last delivery, or of the last abortion, which left the uterus in the sub-involved or unreduced condition which we are considering. The degree or amount of remaining enlargement varies infinitely in different cases; and is, perhaps, mainly regulated by the date of the confinement at which the arrest of the uterine involution takes place, and the completeness or not of that arrestment. I have seen instances where the uterus was large enough to be felt some inches above the pubis, and where the uterine sound passed into the uterine cavity to the extent of four or five inches. In other examples you will find the uterus in form and length remaining hypertrophied only to a slight extent, and the sound passing perhaps not more than two and three-fourths of an inch or three inches. Between these two extremes, you will meet in practice with all intermediate degrees of this permanence of the puerperal hypertrophy of the uterus.

Treatment of the disease.—In the more acute forms of the disease, I have almost invariably found that by the immediate employment of local antiphlogistics the symptoms were speedily and successfully alleviated, and the process of absorption was set up. Where any traces of inflammation still remain, the importance of this indication is at once apparent, and its fulfilment is in most cases attended with rapid relief. But even in cases where all inflammatory action seems to have died out, and only its results remain, we often find—I cannot well tell you how—that a local antiphlogistic course of treatment has the effect of setting up absorption in the enlarged organ, and leading ultimately to its restoration to its normal state. In following out this line of treatment, then, you will do well—if the patient is not very weak and debilitated—to begin with the application of eight or a dozen leeches to the vaginal portion of the uterus, or simply to the perineum, or circle of the anus. I need hardly repeat, that the abstraction of a small quantity of blood in this way will be more particularly called for, and more especially beneficial in those cases where there lingers any degree of congestion or of inflammatory action in the uterus. But even in such acute or subacute cases, and in all the more chronic forms, your chief reliance must be placed on the use of counter-

irritants applied externally to the surface of the abdomen or sacrum. You may use antimonial or croton ointments, or paint the hypogastric surface twice a-day with strong tincture of iodine till the skin becomes tender; but the vesicant which I have found to be most efficacious in exciting the function of absorption in the womb that is now in abeyance, is the ordinary cantharides-blister, especially in the chronic forms of the disease, and when the bladder is not liable to be irritated. You may order a series of small blistering-plasters to be applied for a time over the lower part of the abdomen, or you may paint a succession of small spots—one every second or third day, with blistering liquid until the uterus begins distinctly to diminish in size. I think you will find the latter mode of applying the vesicant to be the simplest and the most certain. The plan I usually follow is to apply the fluid to a patch of about the size of a crown, and then gradually to surround this patch with a number of others of like diameter, one being added every third or fourth day, until the original central point has been surrounded by a ring of five or six distinct blistered spots. At the same time that you are thus trying to excite absorption by the application of counter-irritants to the cutaneous surface, you must endeavour to promote this object by keeping the vaginal portion of the cervix uteri immersed constantly in ointments of mercury or iodide of lead, or bromide of potassium, or other remedies likely to have the effect of stimulating the absorptive process. This you can do by introducing, or making the patient herself introduce, into the vagina one or two pessaries medicated with these substances every night and morning.

But you will find that these local measures are not always, or in most cases, sufficient to effect a cure. You will be obliged to have recourse also to the internal administration of some of the class of deobstruent remedies; and, of these, the most efficacious are the iodide and promide of potassium. I used, at one time, to rely chiefly on the administration of the iodide of potassium. But of late I have employed the bromide much more extensively, for it has this advantage over the former salt, that its use may be kept up for almost any length of time by a patient without her becoming subject to the kind of marasmus which we sometimes find attendant on the prolonged use of the iodide. The bromide of potassium may not only be given with safety for a lengthened period, but it may be administered with confidence as a good tonic, as well as perhaps the best deobstruent in the Pharmacopœia. It may be depended upon as an active stimulant to absorption, besides possessing the property, beyond all other remedies that I know of, of acting as a special sedative on the reproductive organs. In cases of this kind you must administer it in larger doses than usual, making the patient take six, eight, or ten grains of it three times a-day. Sometimes patients suffering from this form of uterine hypertrophy present themselves to you in a debilitated and anæmic condition. Under such circumstances, you need not hesitate to add iron, managnese, or some form of metallic tonic, alone or in combina-

tion, to the more specific remedies, and endeavour, always in the more chronic cases of the disease, by means of good diet and other hygienic means, to raise the patient's general standard of health.

You will occasionally meet with cases of this disease so obstinate, that the process of absorption does not begin, or does not go on to any satisfactory degree, even after you have had recourse to repeated leechings, and have kept the uterus imbedded for a length of time in discutient ointments, and have duly administered all kinds of deobstruents and tonics. In such cases you may follow up the course of treatment which I have sketched for a very lengthened period without producing any appreciable change in the size of the womb, or any marked alleviation of the patient's symptoms. If, however, by any means you can induce the uterus for a time to take on an action of increased growth, you may confidently hope that this temporary hypertrophy will be followed by a process of absorption which will go on, perhaps uninterruptedly, until the organ is reduced to its normal dimensions. Such a transient increase in the size of the uterus you may at any time produce by taking advantage of the physiological tendency of this organ to enlarge and become developed around any foreign body that happens to be lodged with it. You know that the uterus begins to enlarge when stimulated by the presence of an ovum, or of a fibroid tumour, or of a clot of blood in its interior; and in like manner it becomes developed in size when a foreign body, such as a sponge-tent or an intra-uterine bougie, is introduced artificially. By introducing, then, a succession of very small sponge-tents into the interior of the womb, or by making the patient wear for a time an intra-uterine pessary, you can cause the uterus to take on this hypertrophic action; and by afterwards actively and fully taking advantage of the tendency of the organ to undergo a process of degeneration and diminution on the removal of the stimulus, you may succeed, by the due employment of the various discutients and deobstruents I have already mentioned,—as rest, counter-irritation, and bromine,—in promoting the process of absorption to such a degree that the uterus at last becomes reduced to the natural standard. In some obstinate instances I have been obliged to repeat from time to time this process of artificial irritation and dilatation before a perfect cure was effected.—*Med. Times and Gazette, Jan.* 5, 1861, *p.* 1.

100.—ON SUPER-INVOLUTION OF THE UTERUS AND AMENORRHŒA.

By Dr. J. Y. Simpson, F.R.S.E., Professor of Medicine and Midwifery in the University of Edinburgh.

[The condition of super-involution of the uterus is precisely the opposite of sub-involution or permanent enlargement, which is owing to deficient disintegrating process after delivery. In the disease under consider-

ation the size of the uterus is excessively reduced, so as to become smaller than its normal dimensions in the unimpregnated state.]

The hypertrophy resulting from a defective degree of degeneration and diminution of the womb, or *sub-involution*, is one of the most common of uterine disorders. The atrophy that results from excessive resorptive action in the uterus, or *super-involution*, is, on the other hand, a phenomenon of comparative rarity; but that it does ever and anon occur, is a matter to which there can be no dispute. I have seen in practice a very considerable number of cases of this morbid state; but I have rarely had opportunities of examining into its nature on the dead subject. Several years ago I published the history of a case of this kind occurring in a young woman of twenty years of age, who never menstruated after her first delivery, and where the morbid condition was traced, both during life and after death. It was two years subsequent to her confinement that this patient sought admission to our Clinical ward, in consequence of amenorrhœa and great constitutional debility. She was then subject to various distressing symptoms in connexion with the pelvic organs, and stated that she was subject to frequent attacks of diarrhœa, which she believed to be most severe at recurring monthly intervals; the dejections being then sometimes tinged with blood. The mammæ were shrunk and flat; and she was thin, feeble, and anæmic in appearance. On making a vaginal examination, I found the uterus small and mobile, the cervix uteri much atrophied, and its vaginal portion scarcely forming any projection into the cavity of the vagina. The os uteri was so contracted as to admit with difficulty a surgeon's probe. It was dilated by a slender bougie left in it for a few days, and when the sound was subsequently introduced, the uterine cavity was found to be only an inch and a-half in length. Various remedial measures were adopted without any very marked effect; and as the uterine symptoms seemed to be only of secondary importance, she was transferred to another ward, where she died in the course of a couple of months in a state of prolonged coma. During her stay in the hospital she suffered from frequent, and sometimes severe attacks of diarrhœa, and about a month before her death, albuminuria and general dropsy had set in. At the post-mortem examination crude tubercles were found in both lungs. The liver was enlarged, and showed fatty degeneration. The kidneys presented also some stearoid degeneration; and in the right there was a small tubercular abscess. The large intestines were very much thickened in their parietes, and contracted in their calibre; their mucous membrane was ulcerated in various parts. At the lower end of the ileum several large ulcerations were seen running circumferentially around the interior of the bowels. One or two ulcerations were also found in the stomach. The uterus was very small, and atrophied in its length and breadth; its size being diminished about a third below the natural standard in all its measurements, and its parietes were correspondingly thin and reduced.* The whole length of the uterine cavity from the os

to the fundus, was not more than one inch and a-half, instead of two inches and a-half as in the normal condition. When a section was made of the posterior wall of the organ, the thickness of its parietes at their deepest or most developed portion was not above three lines, instead of five or six. The tissue of the uterus appeared dense and fibrous. The ovaries were also much atrophied, and smaller than natural; their tissue was dense and fibrous, and presented no appearance of Graafian vesicles. There was no inflammatory deposit on the peritoneal surface of the uterus or its appendages; but there was some thick pus, or tubercular matter, in the distended cavity of the right Fallopian tube.

In this case we have evidence of the most unmistakeable kind that the uterus of a woman, in whom all the genital apparatus was so perfectly developed as to allow of her giving birth to a fully-formed child, became atrophied to such a degree, subsequently to her delivery, as to be incapable of again resuming its functional activity. How this excessive wasting of the womb is produced it is difficult to understand. Remembering that when inflammation is going on in any organ or tissue, the function of absorption is there in abeyance, we can understand how the process of absorption after delivery should be checked in the uterus, by the occurrence of inflammation in its walls; and how in such a case the womb should be left in a permanently hypertrophied condition, when the absorptive action is not subsequently re-excited. We can even imagine that interruption to the free flow of blood through the uterine vessels, resulting from the patient getting up too soon after her confinement, should lead to a certain degree of congestion in the uterus, and should so far interfere with the process of absorption as to leave the organ in a state of abnormal hypertrophy. But we do not yet know enough of the normal process of involution of the uterus; the reason why the fatty metamorphosis and the subsequent absorption go on exactly to such a point that the uterus is restored almost precisely to its previous dimensions, has not been sufficiently elucidated to enable us to form any probable idea as to the causes which lead to its super-involution and permanent atrophy. Observations even are wanting to show whether this atrophy of the uterus depends upon a morbid degeneration and destruction of fibres that ought to have remained entire; or, as is more probably the case, on some defect in the development of the new cells and fibres that should have taken the place of those that had become effete. A uterus, which has undergone this morbid change, is found after death, as I have said, to be abnormally small. Instead of measuring two and a-half inches or so in length, and one and a-quarter in breadth, it is seen to possess little more than a half, or two-thirds of those dimensions; and this diminution in size affects, not only the body and fundus, but the cervix as well, so that instead of projecting into the vault of the vagina the os uteri seems simply to form a depression in the upper extremity of that canal. The walls of the uterus are at the

same time extremely thin—hardly thicker, in extreme cases, than the coats of the intestines, and so friable as to be readily perforated by the point of a sound incautiously introduced. The whole organ, in short, presents all the appearance of the uterus of a girl not yet arrived at puberty; and differs from it only in this, that whereas the latter is in a condition to become enlarged, and to furnish the ordinary menstrual secretion so soon as a sufficient stimulus shall have been applied to it in consequence of the changes connected with ovulation, the super-involuted uterus has undergone such morbid alteration as renders it unable any longer to respond to the ordinary ovarian influences. And here, again, arises an interesting question in regard to this subject which it remains for future histological research and pathological experiment to answer, viz., what is the cause of this insensibility of the super-involuted uterus to the reflex irritation which seems to be conveyed towards it from the ovaries at each menstrual period? Is it merely that the muscular fibres of the uterus have been so degraded and destroyed as to be insusceptible to the stimulus that usually calls them into functional activity? Or have the nervous filaments that traverse the uterine walls also shared to such an extent in the general atrophy as to be incapable of conducting the influence they ought to convey to the structures in which they ramify? Or have the uterine vessels been so wasted as to furnish an insufficient supply of blood? Or, finally, are the ovaries themselves in a perfectly normal condition, or have they, too, become morbidly altered in structure and impaired in function as a consequence of their intimate association with the diseased and degenerated uterus? These are all questions to which we are at present unable to furnish any adequate reply; but they offer such a tempting field for investigation that I trust some one will ere long try to discover their solution.

Physical Diagnosis.—To ascertain the true state of matters in such a case you must have recourse to a local examination of the uterus itself. On passing the finger into the vagina you feel the cervix uteri unusually small, and quite on a level with the vault of the vagina; or projecting into it so slightly as not to be at once easy of recognition. The womb feels unusually small and light, and moveable; and although the abdominal walls may be very thin and relaxed, yet the organ cannot well be felt between the hand placed over the fundus and the finger exploring per vaginam. The os is small and admits only a narrow-pointed sound; or, as in the case I have detailed to you, nothing larger than a surgeon's probe. In introducing the sound great care must be taken not to press it rudely on, for the walls are thin and very friable; and I have seen a sound thus rashly introduced, and forced with but little effort through the uterine parietes into the cavity of the peritoneum. I have known of this accident happening in more than one instance; and although no bad consequence has ever, so far as I am aware, resulted from it, and although the sound may, doubtless, be passed into the peritoneal cavity with impunity, yet it is

not an experiment one would willingly and wittingly repeat. When the sound, then, is introduced in such a case with all due amount of caution, its point will be found to be obstructed and stopped when it has passed only to a distance of one and a-half or two inches, showing that the organ is preternaturally diminished in size.—*Med. Times and Gazette, Feb.* 9, 1861, *p.* 137.

101.—ON THE TREATMENT OF PUERPERAL MANIA, AT ITS ONSET.

By Dr. J. Y. Simpson, F.R.S.E., Professor of Medicine and Midwifery in the University of Edinburgh.

[As this disease is much more amenable to treatment in its commencement, than when fully formed, we extract that part only of Dr. Simpson's lecture on the subject of puerperal mania and hypochondriasis, bearing upon this period of the disease. It is now an almost established rule, that bleeding is seldom or never permissible in any case of puerperal mania. Supposing there is very great excitement, a few drops of tincture of aconite or of tincture of veratrum viride, may be administered. We must endeavour also to counteract, as far as possible, the super-polarity that exists in the nervous system by the administration of the various]

Nervous Sedatives.—Of these camphor was the remedy most frequently employed by Dr. Gooch in the treatment of puerperal mania; and you will do well to have it in recollection as a remedy that may sometimes prove serviceable. Opium is beneficial in some cases. In others it proves of no avail. You may remember that I called your attention particularly to the great degree of wakefulness that sometimes precedes the attack; and, bearing that in mind, you will see that in some cases it may prove of great service to administer to the patient an anodyne which shall counteract her restlessness, and send her off to sleep. But it is often as difficult to get a patient to sleep who is threatened with an attack of puerperal mania as it is to put to sleep a patient with commencing *delirium tremens*—sometimes even more so. A patient threatened with *delirium tremens*, as you know, usually feels sleep stealing over him in sixty or seventy hours after the commencement of the attack, whether he has got opium or not; but a patient commencing with puerperal mania may remain restless and wakeful for several days and nights before the excitability begins to subside. When once the patient does fall into a good sound sleep, however, her case becomes more hopeful, and the probability is that she will waken up tolerably well. This kind of sleep it must be your endeavour to promote or procure. If the patient is not so violent as to forbid the use of a warm bath, and such a bath be at hand, it sometimes acts as an excellent anodyne in this disease. Medicines are oftener used for the fulfilment of this indication; and

I have seen some cases where an attack of mania has been cut short by a good full opiate administered with this view. The great difficulty is to get the patient to take the medicine. Sometimes she is too mad to swallow it, and too outrageous to be controlled. Whatever may be the way in which you give the drug, remember always, as the grand rule to guide you in its administration to such patients, that it must be given in very large doses. If you expect to have any good effect from it, you must give in general not less than two or three grains of solid opium, or an equivalent dose of some of the cognate preparations. You may give it by the mouth, or in the form of an enema; or when the patient cannot or will not swallow, and will not admit of an injection, you may succeed in introducing the drug in the form of a suppository into the rectum. A suppository containing half-a-grain, or a grain, of morphia is one of the most convenient and efficacious means of administering an opiate, especially for the relief of pelvic or abdominal pains in cases where the stomach is too irritable to tolerate any other preparation of the medicine. But to produce any effect on a patient threatened with puerperal mania, you will require to use a suppository containing perhaps one or two grains of the morphia. Some years ago I was sent for to see a lady about forty miles from Edinburgh, the second wife of a gentleman whose first wife had died of puerperal mania. The lady whom I had had come to see was still in the stage of restlessness, had been chattering continually for some time, and refused to take any kind of medicine, or to admit of the administration of opium in any form. She recognised me when I entered her room, and rebelled at once, declaring she would have none of my remedies. With some little management, however, I succeeded in passing into the rectum a suppository with two grains of morphia, with this result, that in an hour or so she fell asleep, and after sleeping about sixteen hours she awoke up well, and had no recurrence of her maniacal symptoms. In some cases you may get the patient to sleep by bringing her under the influence of ether or chloroform. I have sometimes found that a patient, after being anæsthetised by means of chloroform, has continued to sleep on, and has afterwards wakened up quite well. More frequently, however, she awakes in the same state as when she went to sleep—no better, but also with no aggravation of the symptoms. In all cases, therefore, where such remedies are not contraindicated, you ought to give the patient the chance of recovery offered by the use of opium or chloroform. In using the chloroform, you must see to have the patient fairly anæsthetised and fully asleep; and you must either remain with the patient, or have some competent person beside her, to keep up the anæsthetic condition, by making her inhale a fresh quantity of the drug whenever she gives signs of awaking, as she is likely to do every half-hour or so. In some cases, as I have shown you, you may expect by this kind of treatment to cut short an attack of the disease. I have known the remedy successful in

several instances, producing at once a cure. But this happy result, alas! does not follow in many. In nine cases out of ten, after the patient recovers from the influence of the narcotic or the anæsthetic, the symptoms recur, and the malady continues its progress unchecked. In these—the large majority of cases—therefore, you must seek to combat the disease by other means.—*Med. Times and Gaz., Dec.* 8, 1860, *p.* 550.

102.—PUERPERAL HYPOCHONDRIASIS TREATED BY TINCTURE OF ACTEA.

By Dr. J. Y. Simpson, F.R.S.E., Professor of Medicine and Midwifery at the University of Edinburgh.

When a patient has been much pulled down by hemorrhage, or becomes exhausted by nursing, a state of anæmia or chlorosis, attended by more or less mental depression, want of energy, and loss of memory, particularly of proper names, will supervene, requiring the administration of an improved diet, wine, and tonics, such as iron and quinine. But occasionally an analogous degree of mental misery and depression comes on without any preceding hemorrhage, and when the mother has not acted as a nurse at all. These cases are generally cured by the tonic means I have just alluded to; by change of air and scene, when that is practicable; and in some obstinate instances, where these measures fail, you will find Dr. Seymour's plan of steadily giving an adequate opiate every night, to be a mode of treatment followed by the best results. Of this type of disease I lately saw a very marked case that had utterly defied all the proposed modes of treatment, and that yielded at last with a rapidity which astonished both the patient and myself, under the use of the tincture of the black snakeroot or actea. This plant, the *Actea* or *Cimicifuga racemosa*, has been long spoken of as a remedy for rheumatism, and particularly in the more acute forms of the disease. In the edition of Gray's Supplement to the Pharmacopœias, published in 1821, you will find the use of it in rheumatism stated. Latterly it has been employed by some American physicians as their most valuable remedy in acute rheumatic fever. My very intelligent and excellent friend Dr. Voris, of Rochelle, New York, told me, two years ago, that since employing the tincture of actea in rheumatic fever—and it is a very common disease in his district—he had seen the disease almost always cut short before the eighth or tenth day; the drug acting apparently as a simple antidote to the rheumatic poison, and curing without diuresis, diaphoresis, or any other discharge. The American physicians give a strong tincture of the root in acute rheumatism in doses of thirty to sixty drops every two, three, or four hours. It may be given, if you choose, along with

alkaline salts, or other anti-rheumatic drugs. I have found it, in my own case, repeatedly cure an attack of lumbago with wonderful rapidity. Some of the American practitioners who have written upon actea, have spoken of its use in terms that are, no doubt, exaggerated. Thus. Dr. Davis, of Chicago, says that, after much experience, he has no more doubt of the efficacy of actea in the early stage of acute rheumatism, than he has of the power of vaccination as a preventive of small-pox. But our American brethren have used actea also extensively in chorea and other anomalous forms of nervous disease. However unlike rheumatism and chorea may look to the superficial observer, yet the able investigations of Dr. Begbie and other pathologists have shown, as you are aware, an analogy. if not an identity, between the blood-poison which produces rheumatism and that which produces chorea. Dr. Physick, of New York, and Dr. Jesse Young, of Pennsylvania, about thirty or more years ago, recommended actea strongly in chorea. Latterly, Drs. Lindsey, Kirkbride, Otto, and others have published their experience in favour of the same drug in this disease. In a case of anomalous and severe chorea of long standing, which was under my care some months ago, the actea was given with excellent effect. The patient had been previously treated, both in France and in this country, with zinc, iron, arsenic, and all the usual remdies employed in this malady. But I have made all this long episode regarding the actea, not so much to speak of its use in the preceding diseases, as of its use in puerperal hypochondriasis and depression. A lady, the mother of several children, was twice the subject of the most painful mental despondency a month or two after delivery. On one of these occasions she was confined in London, and had the advice of several eminent physicians; but the disease took a very long and tiresome course, seemed to defy entirely all remedies, and gradually and very slowly terminated. On the last occasion on which the attack occurred, this patient was confined under my care here, and went home to England some weeks subsequently, perfectly well. She returned, however, in about a month to Edinburgh in the lowest possible state of depression, a perfect picture of mental misery and unhappiness. I tried many plans to raise her out of this dark and gloomy state. All failed. At last, fancying from some of her symptoms and complaints, that there might be a rheumatic element in the affection, I ordered her fifty drops of tincture of actea thrice a-day. After taking one dose she refused to continue it, as the drug had a taste so similar to laudanum, and as all opiates had made her worse. On being re-assured that there was no opiate in the medicine, she re-commenced it, without any faith, however, in the results, as she had in a great measure lost faith in all remedial means. When I saw her next, some eight or ten days afterwards she was altered and changed in a marvellous degree, but all for the better. On the third or fourth day, as she informed me, the cloud of misery which had been darkening her existence suddenly began to dissolve and dispel; and in a day or two more she felt perfectly

herself again in gaiety, spirits, and energy. But nothing would induce her to give up the actea for six or eight weeks longer: and the last time she passed through Edinburgh she told me that she had prescribed her own remedy to more than one melancholic subject with nearly as great success as she had used it in her own case.—*Med. Times and Gazette, Dec.* 8, 1860, *p.* 552.

103.—ON THE CONNEXION OF PUERPERAL INSANITY WITH ALBUMINURIA.

By Dr. J. Y. Simpson, F.R.S.E., Professor of Medicine and Midwifery at the University of Edinburgh.

[Puerperal insanity is well known to depend upon various conditions, as, hereditary predisposition to insanity; anæmia; the peculiar state that exists after delivery; and upon toxæmia, or blood-poisoning. The connexion between puerperal insanity and albuminuria, is, however, much less known. Albuminuria precedes and attends the first access of puerperal insanity in a large proportion of cases, but not perhaps so frequently nor so constantly as it precedes and attends upon attacks of puerperal convulsions. The coagulability of the urine generally disappears within a short time after the attack of puerperal insanity commences, in some cases within two or three days of the access of the malady. When the insanity recurs in the form of successive attacks or explosions, each attack is connected with a new attack or advent of albuminuria. How can the presence of albumen in the urine originate two such diverse morbid states as convulsions and insanity? Pathological chemistry cannot yet solve such a question, but]

The presence of an excess of albumen in the urine is usually accompanied with other and far more important changes directly in the chemistry of the renal secretion, and indirectly in the chemistry of the blood itself. When albuminuria exists, there is always liable to accompany it a diminished elimination of the urea, and perhaps of other excrementitious matters that pass off by the urine; and, consequently, there is a corresponding retention within the circulating system of these effete substances that should be duly and systematically thrown off by the action of the kidneys. Urea when thus, or when otherwise, accumulated in morbid excess within the circulating system, does not, according to the observations and experiments of Frerichs and others, produce any very special intoxicating or poisonous effects upon the nervous system, merely *per se*, or simply as urea. But the accumulated urea is apparently liable to decompose and become altered under varying influences into various new organic compounds within the body, some of which, like morphia, strychnine, conine, and other vegetable alkaloids, produce different diseased

states, by exerting different toxicological effects upon the cerebro-spinal and other systems. Dr. Frerichs holds that the mere simple and common decomposition of the accumulated urea into carbonate of ammonia is the cause of albuminuria ending in convulsions and coma. Urea, as urea, does not, in his opinion, and according to his experiments, produce convulsive and comatose effects; but these effects are produced by a sudden excess in the circulating system of carbonate of ammonia, a salt which urea readily forms when it becomes decomposed. But when the whole constitution and chemistry of the blood becomes deranged and altered by the sudden accumulation of the excrementitial materials of the urine within it, as is always liable to happen in puerperal albuminuria, other organic toxicological agents may become developed within the system from the decomposing urea or other components of the urine—possibly some of these newly-formed products or agents of an *alkaloidal* character—and one or other of which may be as certain of exciting delirium and insanity, as an overdose of morphia or brucine, or other poisonous vegetable alkaloids, is certain of exciting their special toxicological effects upon the economy. Mr. Calvert and other chemists have lately found organic alkaloids of various, and most of them of still unknown, types, formed during animal decomposition. In the blood of the puerperal female,—greatly modified as it is in the normal states of pregnancy and delivery, and containing as it does after parturition the effete elements of the involving or disintegrating uterus, and the materials for the new lacteal secretion,—ferments and agents may possibly exist, which are more apt to develope special morbid poisons out of the retained renal excretions than happens in other states of the system. But I repeat, the whole subject is yet quite dark and conjectural, and will remain so till pathological chemistry is able to cast some light upon it.

First Deliveries.—It has been well ascertained that puerperal convulsions occur much more frequently in connexion with first than in connexion with subsequent labours, and that, consequently, puerperal albuminuria is correspondently more common in primiparous than in multiparous patients. Puerperal insanity is apparently also in the same way, and probably for the same reason, but not perhaps to the same extent, observed more frequently after first than after future labours. When speaking of the relations of convulsions to insanity, Dr. Reid quotes authority to the effect "that each of them is more liable to attack the female in her first accouchement than in after ones." Marcé found that among 54 cases of puerperal insanity, 14, or about 1 in every 4, were primiparæ. Out of a list of 53 cases collected by Dr. Gundry, 18, or about 1 in every 3, were found to have been attacked with the disease in connexion with their first pregnancy and first parturition.

Mental Emotions.—We occasionally find, in the case of puerperal

patients, in whom all the circulatory and secreting functions are disordered and deranged, in consequence of parturition and the resulting changes in the generative and mammary organs, that a shock which, in other circumstances would have produced no more dire result than a passing mental emotion, may have the effect of disturbing completely the mental balance, and calling forth all the symptoms of one of the most distressing and dangerous forms of madness. Many years ago I had under my care a patient who had just recovered from her first labour, and had been going on extremely well for about a month, when she was abruptly told by a gossiping old lady whom she received in the drawing-room, that her father had become a bankrupt during the period of her confinement. This distressing news had, previously been most sedulously kept from the patient, and when thus suddenly and senselessly informed of it, she was led back to her bedroom quite insane, and died of a rapid and acute attack of puerperal mania. Another case, of a still more painful character, happened in the practice of a friend here. A lady—one of that rather numerous class of people who have a constitutional horror of mice—had made her husband happy by giving birth to a child, and was making a favourable recovery. A sister-in-law, who had a deep dislike to this lady, sent her a few days after her confinement a box, which she opened for herself in bed, in the expectation of finding a present for her infant, when she was literally "frightened out of her wits" by the escape of a lot of living mice; for such was the cruel gift of her evil-minded relative.

The forms which insanity usually assumes in the puerperal female are those of mania and melancholia—most frequently the former. But there is nothing special in the character or symptoms of puerperal insanity as distinguished from other forms of insanity. The symptoms, when the disease is established, are simply those of common mania or common melancholia, or of some intermediate type of mental disorder. Before the attack the patient is usually for a time restless and irritable, and does not sleep. If you have a puerperal patient complaining of great or total want of sleep, watch her narrowly and anxiously; for she may speedily lapse into insanity. If, in addition to this wakefulness, you discover the urine to be albuminous, the probability of insanity impending over her becomes only the greater. The first supicious symptom of the actual disease generally consists in some oddity of thought, or of expression, or of affection. I lately saw a case in the country where the patient's insanity began with her insisting upon her doctor, at one of his visits, baptizing her infant. She is markedly different in some of these points from what she is naturally. She begins betimes to utter nonsense, or to talk very volubly, and falls at last into a state of complete delirium, when, unless she be constantly watched, she may make some unhappy attempt on her own or another's life. Or, again, the patient may

begin by being low-spirited and dull, refusing her food, and taking no interest in her offspring, or even showing an aversion to it; she takes an unreasonable and unnatural dislike to her husband, her nurse, or her medical attendant; or harbours peculiar suspicions regarding them or regarding her food, fearing that she is to be poisoned or otherwise killed, and gradually settles down into a state of confirmed melancholia.—*Med. Times and Gazette, Nov.* 10, 1860, *p.* 446.

104.—ON THE INSTRUMENTS EMPLOYED IN THE OPERATION FOR THE CURE OF VAGINAL FISTULA.

By William Buxton Hilliard, Esq., Instrument Maker to the Glasgow Royal Infirmary.

[The operation for the cure of vesico-vaginal fistula is one which has now been frequently performed, but it is acknowledged to be one of a tedious and troublesome nature, trying the endurance of the sufferer and the patience of the operator. Mr. Hilliard says:]

The first successful operation in this city for the cure of vaginal fistula was performed by Dr. Bozeman, who had had much experience, having frequently operated on similar cases both in this country and America; the operation occupied an hour and a-half, which appeared a longer period than the work executed should require; but it was not a want of skill or dexterity in the operator that protracted the operation, nor was it any untoward occurrence or opposition of the patient that caused the delay. I thought then, as now, that the instruments which Dr. Bozeman employed, though devised expressly for the operation, were not so well adapted for it as could be wished, and that, if other, more suitable, and fewer instruments were used, the operation might be performed in much less time, and probably with better results.

The first cause of delay was attributable to the speculum, which falls far short of satisfying the reasonable demands of the operator; this speculum is usually designated the duck-bill speculum, and is nothing more than a retractor, for it has no dilating power; in employing it to retract the posterior part of the vagina, it draws together the sides of that cavity, and thus affords but a very imperfect view of the fistulous opening, and greatly limits the scope for the surgeon's manipulations. So very defective, indeed, is this speculum, that in most of the operations which I have witnessed, an additional retractor, held by an assistant, had to be employed on each side of the vagina before a proper view could be obtained, or the parts put in position for operating.

In the second place, the operation was protracted by the tedious and uncertain method adopted in paring the edges of the fistula, and

by the unnecessary number of instruments made use of in accomplishing that part of the operation. The plan which Bozeman, and some other operators pursue, is to seize the lips of the fistula with a sharp hook, or with a favourite kind of forceps, and then with knives of various shapes and curves, assisted with scissors, endeavour by "cautious steps and slow," to pare the flaps, bit by bit, until the entire circumference of the abnormal opening is supposed to have been traversed with the knife. I say supposed, for the bleeding which occurs obscures the view, and sometimes so confuses the operator that he can rarely say more than that he hopes he has accomplished this all-important part of the procedure. Surely the number of instruments which are usually paraded before the operator, should enable him to give a more encouraging account of the progress of his operation.

The third cause of delay arose from using a metallic plate with a detached perforated pellet to secure each suture; here there was time lost in threading each of these separate articles, and there was great difficulty in keeping down upon the closed wound the plate and pellets at one uniform tightness, and when time and care were freely bestowed, still it was not unlikely that some of the sutures would be less tense than others, and the operation fail in consequence.

These objections to Bozeman's instruments it may be thought are imaginary, for his operations are generally successful. To this I reply that Bozeman's success is the result of long experience, and that had his instruments been better adapted to the work, his operations would assuredly be less tedious, and perhaps even more successful. In proof of the inefficiency of the instruments, every subsequent operator who has published or made remarks on the operation, has had something to urge against them, and improvements to suggest. Thus other kinds of knives and forceps have been tried, an instrument for twisting the sutures employed, a wire splint for supporting the flaps proposed, a nippled plate instead of the perforated shot, recommended, and best of all a tubular needle invented for the insertion of the metallic sutures. With these improvements I believe the operation is now performed by many surgeons in less time than the first one required. I have frequently seen it executed by Dr. Eben Watson in one-third of the time it then took, and so admirably done that his success, in every case submitted to him, has been hitherto uninterrupted. With further improvements there appears a probability of facilitating the operation yet more, and I have therefore endeavoured to devise a set of instruments, few in number, and of easy application, in the hope of realising so important a desideratum. These instruments may be best described in the order an operator would require them, beginning with the speculum. As a substitute for the duck-bill shaped one, I have designed an instrument of the form here represented, which, as will be seen, is a quadrivalved dilating speculum,

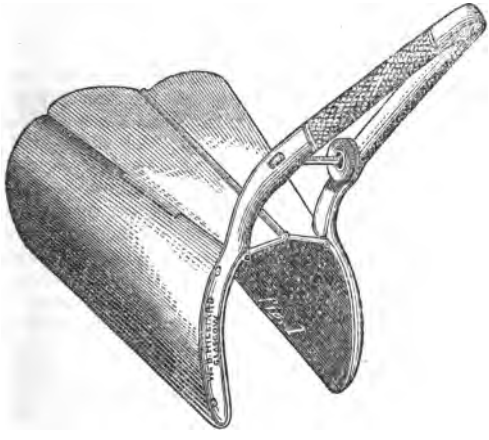
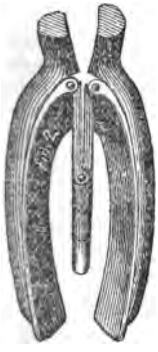


FIG. 1 shows the speculum expanded to its full extent.
FIG. 2 is a full-sized section of the speculum when closed, in which state it is introduced with great facility.



and differs from other specula in that it dilates the interior and orifice of the vagina in normal proportions; other dilators either expand in parallel lines, or if they dilate the interior, they contract at the orifice.

This speculum, I submit, is well adapted for vaginal fistula operations; it elevates what may be termed (in the position in which the patient is placed) the roof of the vagina, and at the same time retracts the walls, causing the part in which the fistula is situated to present a smooth horizontal surface, like stretched canvas prepared for elaborate needle-work, affording to the operator a full view of the fistula, and ample scope for his manipulations. It will also be found, in most cases, a self-retaining instrument, requiring no assistant to hold it in position, or to take the charge of it. This quality is the result of the correct adaptation of the speculum to the interior of the vagina. A good view being obtained, the seizing and paring of the lips of the fistula follows. For the performance of this always tedious and difficult part of the work I have devised an instrument which the surgeon, I hope, will readily discover materially facilitates and insures the efficient completion of this portion of the

operation. For want of a better name, I must designate it a "fistula-clamp," represented in Fig. 3.

With the fork-formed point of this instrument the lips of the fistula are transfixed, and firmly clamped together by passing the sliding rod over the points of the fork as they emerge through the posterior lip (see the dotted lines, Fig. 3); then, by raising the clamp, the edges of the fistula, which are grasped by it, are elevated somewhat above the surrounding parts, and with a straight bistoury, or with a slightly curved knife, here figured, can be instantly pared off in one slice; and an examination of the excised portion left within the clamp will show how admirably this hitherto difficult part of the operation is executed with the aid of this instrument. The edges of the fistula being properly pared in this way, union of the parts is more likely to follow, and the operation to be more frequently successful.

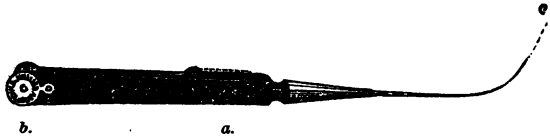
The metallic sutures are best inserted with Startin's tubular needle, having the sliding forceps attached to it, recently invented by Mr. Price—a contrivance which enables the surgeon to slide forward the suture-wire when

Fig. 5. *a.*

FIG. 3.—The fistula-clamp armed with points for a medium-sized fistula. Also moveable extremities of the instrument of different sizes to suit every fistula.
FIG. 4 shows the lips of a fistula transfixed and in the process of clamping together.
FIG. 5.—Fistula-knife. *a*, showing the curve upon the cutting part.

he requires it to project beyond the point of the needle. To this excellent needle I have made another addition, shown in Fig. 6.

Fig. 6.



b. *a.*

FIG. 6.—Startin's tubular needle.
a. Price's sliding forceps for projecting the suture-wire through the point *c*.
b, Hilliard's bobbin, for supplying wire to the needle.

It is a small bobbin affixed to the extremity of the handle, charged with a sufficient length of wire for any number of stitches it may be necessary to insert; with this addition the instrument is a self-feeding needle, and with it the surgeon can apply the sutures in quick succession, without the least assistance from any one.

Fig. 7 is a simple kind of forcep for seizing the suture wire and drawing out a sufficient length to form the suture; it is also a suitable forcep for compressing the nipples of the plate, to be mentioned hereafter.

The lips of the fistula have next to be brought together, placed in correct apposition, and the sutures secured. For this part of the procedure I have devised the instrument Fig. 8, which is both a depressor of the flaps and a twister of the suture. The lips of the fistula are drawn together by passing the ends of each suture through the small aperture *b*. The edges of the fistula are adjusted and the surface regulated by

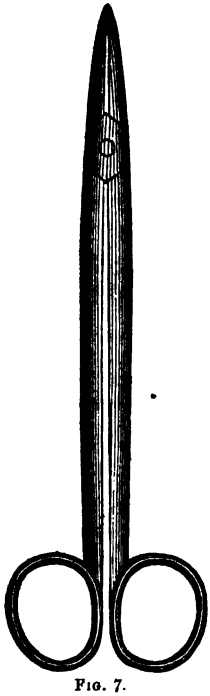
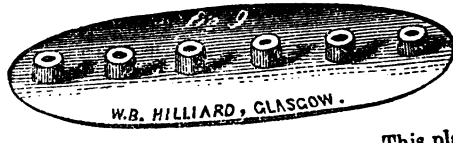


FIG. 7.

FIG. 8.

FIG. 8.—Metallic suture-twister and fistula depressor.
a, Small wheel which rotates the twister.
b. The twister enclosed in the depressor.
c, The suture-wire formed into a loop by the twister.

manipulating a little with the smooth rounded bulb of the depressor, and the suture is securely fastened by one or two turns of the wheel *a*, which twists the suture to the tightness desired: the operation is now, by some operators, thought completed; but many eminent surgeons, desirous of preventing the secretions from coming in contact with the wound, make use of a metallic plate to cover it. The plate which Bozeman employed was secured by lead pellets placed against its upper surface, and there held by being firmly compressed upon the the suture-wires, but on seeing his operation, I immediately devised the plate (Fig. 9), which has nipples upon its upper surface, to supply



the place of the loose pellets used by Bozeman. This plate is found such an improvement that it has entirely superseded Bozeman's plate and perforated pellets. Mr. Baker Brown, I have observed, prefers my single suture-plate, and applies a separate plate for every suture. In doing this he has thought proper to honour the invention with his name. I beg to say that, if there is any merit in the simple contrivance, I certainly have the sole right to it; for the nippled plate was designed by me, and was used, and a description and figure of it published in the Lancet by Dr. Eben Watson, several months before Mr. Brown made mention of it. The whole of the instruments figured and described in this paper (with the exception of the tubular needle) I claim to be my own original designs; the bobbin affixed to the needle is also my own idea; they form a complete set for vaginal fistula operations. I trust their utility may be discovered by the eminent surgeons who have shown a deep interest in the operation, and that these observations will be received by them, and by the profession generally, with indulgence.—*Med. Times and Gazette, Nov.* 24, 1860, *p.* 498.

105.—ON THE OPERATION FOR VESICO-VAGINAL FISTULA.

By I. Baker Brown, Esq.

[Mr. Brown continues with his usual success to operate on these obstinate cases. We give the following in order to show that when a woman is suckling, the operation ought not to be performed without some urgent necessity. It often instructs us better to hear of an unsuccessful case than of a successful one, and we duly appreciate the candour of those surgeons who are not afraid of publishing such cases.]

Mrs. W——, aged 34, admitted April 18th, 1860, into the London Surgical Home, mother of five children.

History.—About three months before her admission she was confined of her last child. The labour was a rather protracted one, and she was attended by a midwife. After the labour she was unable to retain any urine, but gradually improved, and at the time of her admission there was a mere trickling. She was sent to be under the care of Mr. Baker Brown by Mr. Hemming, of Kimbolton.

On examination, there was found a very small fistula at the junction of the urethra with the neck of the bladder, which could hardly be discovered. The opening had originally been much larger, but was now filled up by a very unhealthy loose granulation.

April 26th. The patient being under chloroform, and in the lithotomy position, Mr. Brown performed his usual operation, three bar clamps being used, with iron-wire sutures. She recovered well from the chloroform; but towards the evening unusual sickness came on, which nothing seemed to allay. This continued till the 30th, when she became delirious, and on May 3rd she died, having been insensible for the last twenty-four hours, the cause of her death evidently being pyæmia.

Remarks.—As soon as she was dead, I began to inquire into the cause of so unusual a sequence to the operation. I then ascertained that there was milk in the breasts. This greatly surprised me, as she had assured me that she had weaned her baby some weeks before admission, and she had also led my friend Mr. Hemming to the same belief before he sent her to me. Had there been the slightest doubt in my mind on this head, I should never have attempted the operation until every trace of milk had disappeared, because I had long been satisfied on this head by past experience, especially in one case of death from pyæmia, which followed an operation for ruptured perineum, where milk was still in the breasts.—*Lancet, Nov.* 17, 1860, *p.* 484.

106.—REMOVAL OF WARTY GROWTHS AROUND THE VAGINA AND ANUS OF A PREGNANT WOMAN.

Case at the Middlesex Hospital under the care of Mr. SHAW.

[Can operations be performed with safety on a pregnant woman? The danger is, the induction of premature labour, and this has been known to result even from a tooth being pulled; but, on the other hand, some of the most important operations have been performed on women in the pregnant state, and no untoward result whatever has happened. The rule should be to perform any operation absolutely necessary, to defer it if not so.]

A pale, sickly and emaciated young married woman, of diminutive appearance, was recently admitted into Middlesex Hospital. She was

about six months pregnant, and suffered from a large and prominent mass of warts surrounding the lower part of the vagina, and extending to the anus, which was also completely encircled by them. They had become so agglomerated as to present a tolerably firm epithelial growth. There were in addition numerous small isolated warts on either side of the vulvæ and thighs. All were the result of a former attack of gonorrhœa, existing previously to her pregnant condition. The vaginal outlet was diminished to the size of a few lines, and it seemed to be quite clear, that if she was allowed to go to the full time of utero-gestation, there would be a great risk of serious impediment to labour, in consequence of the state of the parts. Rupture of the perineum, if not some more serious lesion, might be anticipated under the circumstances.

Her case was carefully considered by the staff of the hospital, including Dr. Priestley, the physician-accoucheur, and it was determined to remove all the diseased mass under the influence of chloroform. This was accordingly put into practice on the 28th of November, by Mr. Shaw, who, with the aid of a scalpel and a pair of curved scissors, effectually dissected away and cut off the various parts of the tumour, and all the smaller warts. This was done without any sacrifice of the integuments, for the whole were found to be superficial, having only involved the epithelial layer of the dermis. The anus and vagina were thus freed, and the outlet of the latter, although still a little constricted resumed nearly its natural dimensions. As might be expected, there was free bleeding during the operation, but it was readily controlled afterwards by a piece of lint steeped in a solution of the perchloride of iron.

Dec. 14th. No constitutional disturbance followed the operation and a good recovery was made by the patient. The affected parts are sound, except a few warts which were left, as Mr. Shaw was desirous not to protract the operation, or cause her to lose any more blood. She has been out of her bed now for upwards of a week.—*Lancet, Dec.* 29, 1860, *p.* 632.

107.—ON THE SURGICAL TREATMENT OF FIBROUS TUMOURS OF THE UTERUS.

By I. Baker Brown, Esq., Senior Surgeon to the London Surgical Home.

[Mr. Brown, in the following paper, speaks only of uterine fibrous tumours of the non-pedunculated form, growing from the inside of the uterus by a broad base.]

Case 1.—*Intra-uterine fibrous tumour, of seven years' duration; operation; cure.*—A. E. E——, aged 35, unmarried, admitted into the London Surgical Home, April 14th, 1859, in a state of great prostration and anæmia, complaining of occasional sickness, pain in the

epigastrium, and flooding at intervals of a fortnight, lasting for a week each time. On examination, Mr. Brown found a fibrous tumour, as big as a fist, within the uterus. On the 25th of May, her health being improved, Mr. Brown incised the os uteri, which had the effect of checking the hemorrhage, except at the menstrual periods (which occurred every three weeks.) On Oct. 27th, the patient having been for three months in the country, Mr. Brown gouged a piece out of the tumour. This second operation had the result of breaking up the growth. At every catamenial period she had increased pain, which Mr. Brown ascribed to increased determination of blood to the uterus at these times. This pain and discharge attending the dissolution of the tumour, ceased in Feb. 1860; and on the 21st she was discharged cured. Mr. Brown had heard from this patient that she continues well, takes vigorous exercise, and menstruates regularly both as to time and quantity.

Case 2.—*Intra-uterine fibro-cystic tumour; operation; relief.*—C. N——, aged 30, married, admitted into the London Surgical Home May 19th, 1859. Mother of three children. Had suffered from excessive lochial discharge and frequent hemorrhages since her first confinement, the latter being abated during the time she was suckling, and recurring when she weaned her child. Two years since the birth of the last child she felt a tumour in the abdomen, which has increased in spite of much medical treatment. On admission she was quite anæmic, the abdomen enlarged, and a leucorrhœal discharge from the uterus. Under tonic treatment, &c., the latter disappeared; but she had a severe attack of hemorrhage, continuing in spite of styptic treatment, for nine days.

Oct. 1st. Left the Home better in health, and menstruating regularly.

May 27th, 1860. Re-admitted; extremely weak, and almost bloodless. During her absence from the Home had been pretty well till February, when she lost at one menstrual period three pints of blood; the hemorrhage recurring every eight or nine days till re-admission. Tonic treatment and generous diet.

July 5th. Mr. Brown divided the os uteri; the tumour could be felt just inside it. This operation stopped the hemorrhage, and on the 26th Mr. Brown broke down the tumour with sharp pointed scissors. This had the immediate effect of lessening the tumour, which was passed away in lumps, with an offensive discharge. She had symptoms of pyæmia in August, which readily succumbed to treatment.

Dec. 10th. She left the Home, the tumour exceedingly diminished, and causing no inconvenience.

Feb. 1861. Patient in good health, stronger daily, and able to perform her usual home duties. Menstrual discharge very moderate. Since this paper was written, Mr. Brown saw her. She had had a

fresh attack of hemorrhage, and has since been re-admitted to the Home.

Case 3.—*Intra-uterine fibrous tumour; operation; death.*—J M——, aged 46, unmarried, admitted into the London Surgical Home, Nov. 1859. Has suffered from fibrous tumour twelve years. It is now the size of a six months's fœtal head, causing great uneasiness, and frequent desire to micturate. On examination, the hymen was found almost imperforate; os and cervix normal.

Nov. 19th. Mr. Brown operated in the presence of Dr. Hall Davis and Messrs. C. Mann and P. H. Harper. Mr. Brown divided the os and cervix, and, finding the tumour imbedded in the left side of the uterus as low as the os internum, he cut through the capsule, gouged a piece out, and broke down its tissue. Little hemorrhage.

20th. Rigors occurred, pyæmia set in and progressed, pus was effused into the pleura, and the patient died on the tenth day after the operation.

Post-mortem examination showed diffused purulent infiltration. Concerning this case, Mr. Brown remarked that the fatal termination was due to having gouged the tumour at the same time that he incised the os, and in addition to having broken down the hymen. He had determined in future to divide the operation into two stages.

Case 4.—*Intra-uterine fibrous tumour; operation; cure.*—E. B——, aged 41. married, no children, admitted into the London Surgical Home, Feb. 18th, 1860. Had suffered for four years from hemorrhage and consequent debility. On examination, a fibrous tumour was diagnosed.

Feb. 27th. The os uteri incised.

May 7th. The patient having improved in health, which had been indifferent since the last operation, the tumour was freely incised in its centre, and the vagina as usual plugged with oiled lint. There was an offensive sanguineous discharge for two or three weeks; the tumour gradually diminishing.

June 20th. Catamenia appeared in nearly normal quantity.

July 7th. Left the Home.

Mr. Brown had since heard from this patient. She can take exercise without pain or annoyance, and has no return of the hemorrhage.

Case 5.—Mrs. M——, admitted into the London Surgical Home Nov. 16th, 1860, having been sent to Mr. Brown by Messrs. Alfred Cooper and Paget, of Leicester. Is a widow; had been married eight years and a-half; without children. Has lost much blood at each menstrual period during the last five years, but only discovered an enlargement of the womb two years ago. This had increased rapidly during the last few months, and she had suffered from a white, watery discharge after each period.

On examination the uterus was found completely retroverted, the os being so close to the pubes that it could not be reached. Within the

uterus a fibrous tumour could be felt as large as a fœtus at the fourth month. There was also distinct fluctuation at the posterior wall of the uterus, arising from accumulation of the menstrual fluid. This showed that the tumour was situated on the anterior wall of the uterus.

Nov. 29th. Mr. Brown operated in presence of Mr. Paget, Mr. Alfred Cooper, and many other gentlemen. Finding that the os could not be reached, he made an incision in the posterior wall of the uterus, which was very thin, letting out a quantity of fœtid sanguineous fluid. The incision was carefully carried up to the os and towards the fundus, taking care to avoid wounding the peritoneum; and the fibrous mass could now be seen and felt.

Dec. 29th. The tumour was freely gouged, and consequently greatly diminished.

Jan. 24th, 1861. The tumour was again gouged, as it had not disappeared rapidly enough. The tumour decreased to half its size, there being much muco-purulent discharge.

Feb. 7th. Mr. Brown again gouged the tumour, the same results following the operation. He had no doubt the tumour would eventually under treatment disappear entirely.

Case 6.—*Three fibrous tumours; operation; cure.*—E. P——, aged thirty-seven, unmarried. Seven years before, Mr. Brown had removed a fibrous growth from the uterus about the size of a walnut. Two years ago the patient complained of uterine pains and hemorrhage. A year since, in consultation with Dr. Ferguson, three fibrous tumours had been diagnosed, growing from the inside of the uterus, near to and on each side of the os internum.

Jan. 15th, 1861. Mr. Brown assisted by Dr. Cockle and Messrs. Philip Harper and Wratislaw, cut through each of the tumours. A muco-purulent discharge ensued.

Feb. 15th. On examination two of the three tumours had quite disappeared, and the third was only half its former size. The patient has not suffered from hemorrhage since the operation.

Remarks.—The cases related, having been selected from the case-books of the institution with which Mr. Brown was connected, had consequently been under the observation of other practitioners during the whole course of treatment pursued. Other examples might have been adduced from his private practice, but those related showed—

1st. That this class of tumours may be destroyed by cutting into and destroying a portion of their tissue; in other words, by gouging a portion out.

2nd. That this operation could be performed successfully and safely. Out of the dozen or more cases in which this plan had been pursued, Mr. Brown had lost but one, that related in the present paper.

3rd. That the preliminary division of the os and cervix has in itself the very important advantage of nearly, or quite, arresting the uterine hemorrhage, the most serious symptom attendant on these tumours.

Mr. Brown concluded by adding a few observations on what had been done for these tumours by Dr. Atlee and others, and stated that the principal difference between his plan and that of Dr. Atlee was, that whereas the latter gentleman makes an incision through the capsule of the tumour, with a view to its detachment, and to second this purpose, gives ergot of rye, Mr. Brown aims at the disintegration of the tumour, by cutting into its centre, and breaking it down, thus setting up a process of ulceration, but making no attempt to separate the tumour bodily from the uterine wall.

Immediately after the paper had been read, Mr. Brown showed the instruments which he had been in the habit of using in the operation described in this paper. They very much resembled an ordinary centre-bit, twisted in a spiral manner, with various cutting ends. He had experienced great difficulty in getting those instruments to hold, so as to gouge a piece quite out, and had thought something was required to hook the tumour while it was being cut. This idea had been very ably carried out by his friend and colleague, Mr. Philip Harper. This gentleman's instrument consisted of a hollow tube of steel, with cutting knifes. Contained in this tube was a hook, which could be pushed through by a spring, and thus grasp the tumour, whilst the circular knifes were carried through it by means of a screw. In this way a piece was actually cut out of the tumour. The instrument was graduated, so that by measuring the tumour from without, a pretty correct estimate could be formed as to how far the tumour was cut into.—*Lancet, March* 16, 1861, *p.* 266.

108.—MR. PHILIP HARPER'S INSTRUMENTS FOR CORING FIBROUS TUMOURS OF THE UTERUS.

The instruments shown in the annexed engraving are those mentioned by Mr. Baker Brown as having been invented by Mr. Philip Harper, for the purpose of gouging out pieces from fibrous tumours of the uterus. [See preceding article.]

Fig. 1 shows the instrument as it appears before introduction against the tumour. It consists of a hollow tube of steel nine inches in length, graduated at intervals of an inch, so as to show the depth to which the tumour is penetrated. It is open at its upper extremity, but closed at the lower by a movable nut, B. Through the centre of this nut passes a hollow rod of iron, E, having at one end a handle, F, and at the other two knives, either straight, as at H, Fig. 2, or one straight and the other bent at right angles, as at H Fig. 3. At a point on this rod, ten inches from the points of the knives, is placed a male screw, C, working through a female screw in the centre of the nut B. Passing through the tube E is a rod of steel having at its upper end a double hook, as shown at H in Fig. 2. Connected with this rod also is a strong spring, contained in the interior of the instrument, and which therefore cannot be shown.

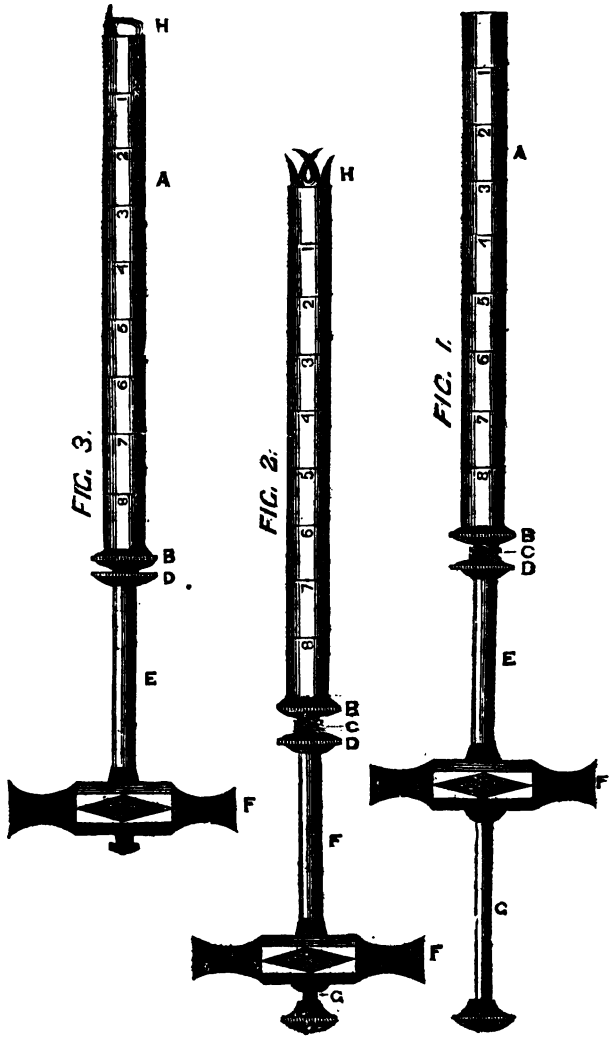


FIG. 3.
H
A
B
D
E
F
FIG. 2.
H
B
C
D
F
F
G
FIG. 1.
A
B
C
D
E
F
G

The mode of its action is as follows:—The instrument in the state shown in Fig. 1 is held in the left hand, passed up the vagina, and placed against the tumour. The screw rod G is then pushed and turned, when the hooks H protrude, and fix themselves into the tumour. The spring acting upon them, draws upon the mass, and tends to bring it into the hollow of the instrument. The handle F is now turned, when the knives gradually protrude, and at the same time cut into the tumour. The depth to which they penetrate is regulated by the nut D, previously fixed at the required distance. When it is desired to penetrate to a greater depth than one inch, after the nuts D and B are brought into contact by repeated turns of the handle F, the movement is still continued, when the whole instrument turns; and as its upper extremity is carefully beveled off, it passes into the incision made by the knives. Thus a piece of any length from one to nine inches can be taken away.

Fig. 3 represents another form of instrument, adapted for that class of tumours which are brought into view easily, and when, from their position, there is no difficulty in reaching them. The difference between this instrument and the one already described consists in the absence of the hooks and rod G H, and in one of the blades of the knives being placed at right angles instead of straight. All the remaining parts of the instruments are similar, and the letters refer to the same parts in each. The curved blade is made very strong, and is placed in such a cutting angle that it acts somewhat as a hook as well as a knife.

For the sake of showing the screw C. each figure differs somewhat. In Fig. 1 the screw is exposed to its full length. In Fig. 2 it is shown after the handle has been turned, and the knives protrude. In Fig. 3 the knives are out to their full length, and, the nuts B and D having met, the screw has disappeared from view.—*Lancet, March* 23, 1861, *p.* 292.

109.—OVARIOTOMY; REMARKS ON THE OPERATION, AND ON THE CAUSES OF ITS MORTALITY.

By Dr. W. Tyler Smith.

[Among the statistics adduced in the commencement of his paper (read before the Obstetrical Society of London), the author observed that out of 395 cases of completed ovariotomy, 212 recovered, and 183 died. This is much more favourable than the results from tapping and injecting with iodine. After relating four of his own cases, the author remarked:]

The operations were performed by the short incision, the cysts being tapped and brought through the wound. The ligature was allowed to fall into the pelvis. A very moderate amount of opium was given, and stimulants were administered on the third or fourth day, in the absence of inflammatory symptoms. Great care was

taken to guard the patients against any miasmatic or contagious influence. The author preferred the ligature to the clamp, on account of the straining caused by the latter in the event of vomiting or tympanitis. As far as statistics are known, the proportion of recoveries is greater when the clamp is not used. The only objection to the ligature is the length of time before it comes away. The following remarks on the causes of mortality, and the means of diminishing it concluded the paper:—

"Nearly 17 per cent. of the fatal cases have occurred from shock or collapse. This source of mortality should be met by resorting to the operation before the patient has reached a state of exhaustion. It is not in many cases decided on till the patient is in such a state of weakness as to be unable to bear this or any other operation. Except in cases of extensive adhesion, there is nothing necessarily belonging to the operation which should produce dangerous shock. This source of danger will be diminished when the operation becomes generally recognised as an established and necessary proceeding in suitable cases. Improvements in diagnosis will doubtless lessen the number of cases in which the abdomen is opened without finding ovarian tumours. Earlier operation would also lessen the number of cases in which adhesions occur.

"Exactly 16 per cent. have died from hemorrhage. All or nearly all of these may be prevented by care in tying the pedicle, and in securing hemorrhage from the separation of adhesions. The ligature should never be left until we are quite sure that all bleeding has ceased. One source of the bleeding has been the use of the ligature without transfixing the pedicle. There is a notable increase of mortality where the pedicle is tied in one mass, as compared with transfixion and tying in two or more portions.

"Peritonitis is by far the most fatal complication which can occur. Not less than 43 per cent. of the fatal cases have died in this way. Looking to the histories of the cases thus lost, it can, I believe, be shown that peritonitis does not depend so much on anything inherent in the operation itself, as upon miasmatic or contagious influences. This seems proved by the great mortality after the operation in hospitals as compared with private practice. It is difficult to get a successful result in a large hospital, yet the operations are nowhere more skilfully performed, nor the after treatment better. Peritonitis following ovariotomy is evidently in the majority of cases not simple inflammatory peritonitis, but a disease closely resembling puerperal fever, and like it commonly due to some external poison. This is the only way in which the excessive mortality after the operation in hospitals in this country and on the continent can be explained. The ovariotomy patient is as susceptible as the puerperal woman, or even more so. Such cases cannot safely be collected together or mixed with other patients; and in the long run, I believe the results cannot be as favourable either in general or special hospitals as under other cir-

cumstances. In the operations detailed every possible care was taken that they should be performed under good sanitary conditions; that as few persons saw the patients or were present at the operations as could well be; and that nothing came near them in the shape of contagion or infection. Each patient was treated, in fact, as we should treat and guard a lying-in woman, and to this, more than to anything else, I attribute their success. By care of this kind we may probably diminish this the greatest source of mortality to a considerable extent. The three causes which I have dwelt upon—namely, shock, hemorrhage, and peritonitis—represent upwards of 75 per cent. of the total deaths after ovariotomy."

Mr. BAKER BROWN congratulated the author on his great success, and also on his having become a convert to ovariotomy, and hoped that many other hospital physicians and surgeons, who had hitherto opposed the operation, would follow his example. He (Mr. Brown) observed that the author had only mentioned two plans of treatment—namely, tapping simply, and tapping with injection of iodine. Besides that of complete extirpation, there was, however, another which he (Mr. Brown) had for many years advocated, and which had received the sanction of many members of the profession—namely, tapping and pressure. This mode of treatment was only applicable to monocystic forms of the disease. He could mention many cases of several years' standing, which had been cured by this treatment. In one particularly, where the lady afterwards married, and has since been delivered by him of four healthy children. In the last year he cured three patients in the London Surgical Home, and there was another, apparently cured, still under observation. He thought that the author had not fairly stated the advantages of tapping with injection of iodine, which in unilocular cysts has been often used with advantage. He had himself reported some cases of cure, as also did Mr. Spencer Wells about two years ago. As regards the administration of opium after the operation, he (Mr. Brown) had, as the author observed, been in the habit of giving opium in all cases, as had also been done by Dr. Clay, of Manchester; but latterly he (Mr. Brown) had been guided in this matter by the pain, proportioning the amount of opium to the severity of the pain, which he regarded as a physiological evil to be removed if possible. He generally gave it by the rectum, as in many cases it does not cause the same amount of sickness as if given by the mouth. He then alluded to the length of time the ligatures had remained in the abdomen: in one case seven, in another case eleven weeks. He thought that this must have arisen from one of two causes: either that they were not tied tight enough, or that too much of the peduncle had been taken up by each ligature. The average time in his experience, and in that of Mr. Lane, had been from nine to ten days. He (Mr. Brown) could not agree with the author in his exclusive use of the ligature, allowing the peduncle to drop into the abdomen. The practice that he adopted was, to use

the ligature where the peduncle was very short, and could not be kept outside the opening without dragging too much upon the uterus; on the contrary, where the peduncle was long enough, he preferred securing it by clamp or otherwise on the outside, as the wound healed quicker and the patient was sooner convalescent. The objection to the weight of the clamp was obviated by using a pair of common carpenter's calipers. These could generally be removed on the second or third day. In two cases lately, in which he had operated successfully in the London Surgical Home, he had removed them on the third day. In reference to the sutures, he thought that when the author had tried the silver wire sutures, he would find them far better than the silk; they were more readily applied, and three or four deep ones were sufficient without the interrupted sutures. He thought that the success of the author was due in a great measure to the fact that none of his cases had been tapped, except one, and that only once, as he believed that the best cases for operation were those which had not existed long, and in which the abdomen had not been disturbed by repeated tappings, and the constitution drained by the constant refilling of the cyst. He thought that the paper of the author would do much good towards a fairer trial of ovariotomy, and that the difficulties which the old operators had hitherto had to encounter amongst the great majority of the profession would be materially lessened by the addition of such an authority.

Mr. SPENCER WELLS said he concurred with Mr. Brown in the opinion that the author had rather undervalued iodine injections in cases of single cysts. He (Mr. Wells) had employed iodine in five such cases, and four of the patients had remained without refilling. In the fifth, the injected sac had either refilled, or others had grown. In compound cysts he quite agreed with the author in regarding iodine as worse than useless. He differed entirely from Mr. Brown in the opinion that pressure was of use after tapping. Doubtless some single cysts had remained without refilling after this treatment; but tapping alone was quite as useful without as with pressure. The pressure could have no effect on an empty collapsed cyst. In two cases in which he (Mr. Wells) had been prepared to inject iodine he had trusted to simple tapping, because, in each case, he found the fluid so clear and limpid that he believed the cyst was not truly ovarian, but one of those thin-walled, simple cysts of the broad ligament which collapse when emptied, and do not fill again, whether pressure be employed or not. As to the position of the patient during ovariotomy, he had tried, in three cases, the sitting posture as recommended by the author; but he very much preferred the recumbent position. The chloroform was less likely to induce faintness; there was less tendency to escape of intestines, greater facility in securing the peduncle or bleeding vessels, and it was not difficult, by holding the cysts well forward as they are emptied, to prevent the ovarian fluid from entering the peritoneal cavity. He quite agreed with what

the author said as to the extent of incision; and partly so as to the mode of securing the peduncle. After using the clamp in four or five cases, he had abandoned it, except as a very convenient and rapid mode of securing the peduncle for a few minutes while the tumour is separated and the wound closed. Ligatures may then be used below it, and it may be at once removed. In the two cases of tetanus, he did not believe the clamp had anything to do with this occurrence. In one case the tetanic symptoms did not appear till the fourteenth, and in the other till the eighth, day after operation; yet the clamps came away on the third or fourth day. The objection to the fixing the peduncle to the abdominal parietes suggested by the author, that increase of the uterus during pregnancy might be interfered with, was not borne out by a case in which a patient upon whom he had so fixed the peduncle had borne a living child thirteen months after ovariotomy. He differed very strongly from the author in his practice of leaving the tied stump of the peduncle within the peritoneal cavity. He (Mr. Wells) had been obliged to do this in two cases out of the twenty-one in which he had operated. One of these patients recovered, but her state was very alarming for some days, and convalescence much more protracted than in any other he had seen. The other died thirty hours after operation with all the symptoms of putrid infection, and he had no doubt such putrid matter as may be seen to come away when the peduncle is kept outside the wound was absorbed from the peritoneal cavity, and poisoned this lady. The length of time occupied by the separation of the ligature when it was left inside, was a great source of anxiety to both patient and surgeon. In some recorded cases, it had remained from thirty to forty days; whereas, when fixed outside, it separated in three or four, or at most eight or ten days, and then the wound healed, and all anxiety ceased. He thought the profession greatly indebted to the author for bringing forward these cases, as they would strengthen the hands of those who were endeavouring to prove that ovariotomy has at least been as successful as several other important operations, and a means of restoring many patients from a state of hopeless suffering to one of perfect health.—*Lancet, Feb.* 16, 1861, *p.* 166.

110.—REMOVAL OF OVARIAN TUMOUR.

[At a meeting of the Obstetrical Society of London lately, Dr. TANNER exhibited an ovarian tumour weighing about 8lbs, which had been successfully removed eleven days before, the patient doing well.]

With regard to the operation itself, there was one point deserving of mention,—namely, that after applying ligatures to the pedicle, the tumour, instead of the pedicle, was cut through, and a portion about the size of a small hand left attached, which was retained outside the abdomen. By this proceeding the use of the clamp was rendered

unnecessary, while all risk of hemorrhage was avoided. The latter point was of no little importance, since bleeding from the pedicle had been the cause of death in no less than 16 per cent. of the fatal cases. The edges of the wound were carefully brought together with twine sutures; these latter not being passed through the peritoneum.

In reply to a question from Dr. Tanner,

Mr. SPENCER WELLS said he had attached the greatest possible importance to the practice of passing the metallic sutures or hare-lip pins through the peritoneal edges of the wound. He had been led to originate this practice by observing the inner aspect of the wound in the first fatal case of ovariotomy which had occurred in his practice. The sutures had been passed through integuments and muscle, but not through the peritoneum; and the consequence was that a raw surface of considerable breadth was only partially protected from the general peritoneal cavity by a portion of intestine, which was already adhering to it, although the patient died the day after the operation. Had she lived longer, there could be no doubt that pus or other secretion from the wounded surface would have been poured into the cavity of the peritoneum. Struck by this observation he (Mr. Wells) made a number of experiments, in concert with Dr. Richardson (who narcotized the animals by puff-ball smoke or chloroform), upon guinea pigs, rabbits, and dogs, opening the peritoneum and closing the wound in some cases by sutures which included the membrane, and others which only passed near it, and killing the animals at various periods after the operations, which were made as accurately comparative as possible. The result was that nearly all the animals recovered; but in those where the more superficial sutures were used there was a portion of the parietes left destitute of peritoneum, and this defect was supplied by adhesion of intestine or of omentum. In two cases where omentum became adherent, the motions of the dogs were materially interfered with. In those cases where the peritoneal edges of the wound had been included, the union was so perfect that it was difficult to detect the line of union two or three weeks after the operation. This was explained by the observation of a case in which a woman died within two days after ovariotomy. The hare-lip pins which had passed through the peritoneal edges were quite concealed from view and protected from contact with intestine, partly by folding of the peritoneal edges together, and partly by effused lymph. Mr. Wells said he had preserved many of the most striking specimens, and would be happy to show them to any gentleman who was interested in the subject.

Dr. TYLER SMITH had recently attended a case which strongly exemplified the good effects of bringing the edges of the peritoneum together by the sutures. After union of the external wound and the removal of the sutures, suppuration took place along the whole line of incision, extending through the entire thickness of the abdominal wall. The peritoneum had, however, healed so perfectly that no pus found

its way into the peritoneal cavity. In this case, silk sutures had been employed. The patient recovered, but this would have been hardly possible unless the edges of the peritoneum had been brought together, so as to produce union by the first intention. It was unnecessary to pierce the edges of the peritoneum to effect this. It was sufficient to take up the tissue immediately above the peritoneum at the edge of the incision, without passing the needle through the peritoneum itself.

Mr. Spencer Wells said that when metallic sutures were used, and the deep ones were removed on the third day, he had never seen any suppuration in their track.—*Lancet, Dec.* 15, 1860, *p.* 585.

111.—SUCCESSFUL CASE OF OVARIOTOMY.

By Dr. D. Lloyd Roberts, St. Mary's Hospital for Diseases of Women and Children, Manchester.

[The patient was 35 years of age, married, and had had three children. The tumour had been noticed fourteen months, it was distinctly moveable in the abdomen, the patient supporting it with her hands when she turned in bed. Before the operation was performed the tumour was twice tapped. Chloroform being administered,]

The operation was commenced by making an incision three or four inches long, from one inch below the umbilicus downwards. The peritoneum was in this manner exposed, and then divided on Key's director: some ascitic fluid now escaped, but not much, as she had been tapped only fourteen days before. The peritoneum covering the tumour was next carefully divided on the director, and a large cyst came into view. This was opened by Mr. Spencer Wells' large trocar, and about 10 pints of thick mucilaginous fluid were rapidly drawn off. It was now perceived that the remaining, and by far the larger portion of the tumour was solid, only a few small cysts being scattered here and there on its surface. I passed my hand carefully round the tumour, and found that there were no adhesions.

My friends, Dr. Radford, Dr. Stephens, and Mr. Lynch, the two latter of whom were kindly officiating as my assistants, here suggested that the incision should be enlarged, as it seemed altogether insufficient for the passage of the tumour; accordingly, it was extended above and below, to the length of nine inches. Through this opening I drew the tumour, which was then firmly held by Dr. Stephens and Mr. Lynch. The pedicle, about four inches long, and an inch and a half broad, was tied by being transfixed twice by strong ligatures, and was then divided close to the tumour. The surface of the peritoneal cavity was now carefully cleared, by means of a sponge, of some fluid and clotted blood which adhered to it; the left ovary and the uterus were next examined, and found healthy; and the wound was carefully closed by means of four silver hare-lip pins, passed through the divided

edges of the abdominal parietes, including the peritoneum, and the adaptation of the edges still further secured by superficial sutures of silver, which were placed in the intervals between the pins. The pedicle was brought outside, and secured by passing a superficial suture through it and the integuments, to prevent its receding into the abdominal cavity. Strips of plaster were used to steady the ligature, and the skin was also protected from the edges of the pins, in the same manner. Strips of plaster extended across the abdomen, and the application of a pad of cotton-wool and a flannel binder, completed the dressing. The patient was put to bed, expressing herself as very comfortable, and "wishing to know if the tumour had been removed."

The tumour exclusive of the contents of the cyst, which had been tapped, weighed sixteen pounds; the weight of the fluid contents of the cyst was twelve pounds; making altogether twenty-eight pounds, as the weight of the tumour in its entire state. It was a good specimen of the pseudo-colloid variety of ovarian tumour, so well described by Spencer Wells. An incision into it showed it to be made up of a number of chambers, varying in size from a filbert to that of a clenched hand, and filled with two different kinds of fluid; one kind presenting the appearance and consistency of very thick mucilage, the other resembling prepared chocolate; the parietes of each chamber consisted of a layer of firm fibrous tissue.

I made a careful microscopic examination of the fluid, but could find none of the morbid cells which are characteristic of malignant growth, but only a number of non-nucleated cells, aggregated together, so as to form corpuscles three or four times the size of blood-discs, and having the appearance of fatty granules.

An hour after the operation, the abdomen was covered with a linseed-meal poultice. At 5 p.m., after four hours' sleep, the patient awoke, and expressed herself comfortable, but fatigued, and rather sick.

In the evening, she vomited a little, the effect probably of the chloroform; at 11 p.m., a morphia suppository was introduced into the rectum; pulse, 104; skin, moist. She passed a good night.

[The progress of the patient was satisfactory, and the last report states that she had much recovered her health and strength.]

I believe that this poor woman owes the preservation of her life to an adherence to the following principles, laid down by Mr. Spencer Wells, viz.:—

First. In making an incision the smallest possible compatible with the passage of the tumour. I may here remark, that all the cases I have seen in which the large incision, "from sternum to pubes," was used, proved fatal.

Second. In bringing and securing the pedicle, external to the incision, instead of leaving it to slough (as was the practice heretofore), within the abdominal cavity. It has fallen to my lot, on more than one occasion, to witness the death of patients about the third week

after ovariotomy; the result, as there is the strongest possible reason to believe, of pyæmia,—the abdomen, on a post-mortem examination, having been found filled with pus, which could be traced to no other cause than the sloughing pedicle.

Third. In the use of silver pins passed through the abdominal parietes, so as to include the peritoneum, thus securing union, by first intention, of the deep parts of the wound, including the divided edges of the peritoneum; and so, by closing the peritoneal cavity, securing it against the entrance of pus, which might otherwise fall into it from any superficial suppurating portions of the wound.

It is my opinion, also, that part of the successful result is due to *the very free* administration of nutriment and stimulants, "per rectum," when the patient was unable or unwilling to swallow them, and at a time when death appeared imminent from the want of them.

I would draw the attention of my professional brethren to the remarkable extent which nutriment and stimulants are capable of being absorbed into the system when administered by this channel.

Great relief was also afforded by the substitution of a light chamomile-bag, covered with oiled silk, instead of the heavy linseed poultice; the latter having occasioned the patient much discomfort by its weight. I have already mentioned that the opiate injection was the only remedy which was successful in checking the obstinate vomiting.—*Dublin Quarterly Journal, Feb.* 1861, *p.* 43.

112.—SPONTANEOUS CURE OF OVARIAN TUMOUR(?)

By JOHN FOX, Esq., Weymouth.

[The patient, who was 22 years of age, had perceived a steady increase in her size for two years, no pain experienced. There was evidence of the presence of a large quantity of fluid.]

She gradually increased in size, until it was evident that there could not be less than from thirty to forty pints of fluid. On the morning of the 25th July, she suddenly felt considerable pain about mid way between the end of the sternum and umbilicus, which she described "as if a person's hand were inside twisting the parts." She then became rather faint and weak. These symptoms soon subsided, and she afterwards took a good deal of exercise, and in the evening took her part in a dance with a few young friends. Suddenly, an urgent desire to empty the bladder was felt, and she passed at least two quarts of fluid, and at the end of about two hours a similar quantity, and so on greater or less quantities until the following Saturday morning (July 28). She then found all trace of enlargement gone, and the integuments of the abdomen lying in loose folds, as they would do after tapping. I then saw her again, and found her weak but cheerful, and free from pain. She assured me that not a drop of urine had passed involuntarily; that she had perfect control

over her bladder, and that the whole of the fluid was limpid and almost odourless. After a careful examination of the abdomen, I could not detect the slighest trace of tumour or even of thickening, and the vagina and uterus appeared to be perfectly healthy. I ordered her the tincture of sesquichloride of iron, and a generous diet with wine, and after a few weeks sent her to see Dr. Lee again. He also failed to detect any tumour or lesion of any kind, after a most careful external and internal examination, and pronounced her to be quite well.

Remarks.—This case suggests several questions. Could it have been an ovarian dropsy finding its way through the Fallopian tube and uterus? I think not; as during the intervals of passing urine she was sure that not a single drop had passed from her. Was the tumour ovarian? and did the cyst give way at the time when she felt the pain and faintness, thus converting the case into one of ascites? If so, could the whole of the fluid have been taken up by the absorbents, and conveyed through the kidneys into the bladder in the short space of between fifty and sixty hours? I think this the most probable explanation; and it agrees with the description of a case mentioned by Dr. Blundell, in which a lady affected with ovarian dropsy had a large discharge from the bladder after striking her abdomen against a stone. When she died, some years afterwards, with retroversion of the uterus, it was found that an ovarian cyst had burst into the peritoneal cavity, and that its contents had been absorbed. Also with a case, No. 20, related by Mr. Beaumont, of Gravesend, in which the ovarian dropsy disappeared after rupture of sac, and passed off by kidneys and bladder. (See Clinical Memoirs on Abdominal Tumours, by the late Dr. Bright, page 121.) Whilst Kiwisch, in his chapter on Diseases of the Ovaries, translated by John Clay, mentions rupture of the cyst into the peritoneum, followed by profuse diuresis; and Churchill refers to rupture through the bladder. The patient now wears a belt, and continues in the enjoyment of good health. She has gained flesh and strength, and is able to walk several miles daily.—*Brit. Med. Journal, Dec.* 22, 1860, *p.* 996.

113.—CASE OF OVARIAN DROPSY TREATED BY THE INJECTION OF IODINE.—SUBSEQUENT EVACUATION INTO THE RECTUM.

Under the care of Dr. DAVIES and Mr. CURLING, at the London Hospital.

Mrs. W., aged 25, was originally admitted under care, at the London Hospital, in July 1858. She was of fair complexion and well-formed. Her disease, which was ovarian dropsy, was believed to have commenced in the summer of 1856. She had previously borne one child. The first ovarian enlargement was mistaken for pregnancy, menstruation being for a time suspended. The enlargement of the

eyes was gradual, and at the time of her admission, two years from its commencement, the distension was somewhat more than that of the full period of pregnancy. Her health was a good deal undermined.

July 20, 1858. Having been in the hospital a few days, paracentesis was performed, and fifteen pints of a greenish-yellow fluid were withdrawn. After the tapping the topical application of iodine was tried and tonics were given. She left the hospital in ten days.

Having remained at home about nine months, Mrs. W. was admitted for a second time in April 12, 1859, being now much larger than ever. Paracentesis was performed on the 14th, and thirty-five pints of brown fluid were withdrawn; the fluid on this occasion was thicker than on the previous one, but not much so. She recovered well from the operation, the cyst, however, commencing to refill almost immediately. She left the hospital on the 28th.

After the second tapping, her husband, who had been in India, returned home; and as shortly afterwards the catamenia ceased, she supposed herself pregnant. Her abdomen enlarged very considerably. While in this condition she had a fall and struck her abdomen on a plank, after which she became suddenly very faint and ill. The accident happened in the country, and the surgeon who was called in performed paracentesis and withdrew thirty-nine pints of fluid.

During July the woman was again in the London Hospital, but she declined on this occasion to submit to another tapping. In August she was re-admitted, her condition having become unbearable. A fourth paracentesis was performed, and thirty-eight pints of dark brown fluid were removed. After this tapping she remained in a very low state for some time, and left the hospital at her own request, apparently about to sink under her disease. She remained at home six weeks, being confined to bed the whole time, but having been somewhat improved in general strength.

August 27th, re-admitted. She was now in a very feeble state, unable to stand, her legs swollen and painful, countenance swollen and anxious. Face thin and drawn. Her abdomen was very large indeed. It was now determined in consultation that treatment by the injection of tincture of iodine should be tried. On October 18 Mr. Curling accordingly tapped the cyst for the fifth time. Forty-five pints of thick brown fluid having been withdrawn, and the cyst having apparently been emptied, a quantity of the pharmacopœial tincture of iodine was injected. After the injection she passed a good night, and did not suffer very much pain. On the next day she said she could taste the iodine. During the following week the urine was frequently examined, and found to contain iodine. The cyst rapidly re-filled, and on the 20th she was almost as large as ever. On the 27th Mr. Curling again performed paracentesis, removing twenty-seven pints. A solution consisting of a drachm of iodine, a drachm and a-half of the iodide of potassium, and three ounces of water, was injected, and

allowed to remain in. About eight hours after the injection, she complained of a burning pain in the lower part of the abdomen, but no severe symptoms followed. During the next fortnight she rather improved in general condition, but the abdomen rapidly increased. On the 17th, whilst sitting on the close stool, she felt what she described as "something hard pressing into the rectum," and the sensation was followed by a snap and a gush of fluid. Twenty-seven pints of thick light-coloured fluid, like gruel, escaped per anum. She became faint, and it was necessary to give brandy, after which she quickly rallied. On the day following about a pint and a-half were passed. On the 20th about a pint escaped, and during the following week the quantity of discharge varied from one to two pints daily. She improved in health slightly, but towards the middle of December a tumour was discovered in the region of the left ovary. From this time she gradually sank, the discharge per anum steadily continuing. Death took place on December 25, eighteen months after the first tapping, and three years and a-half from the date of the commencement of her disease. Unfortunately no post-mortem examination was permitted.—*Med. Times and Gazette, Feb.* 9, 1861, *p.* 143.

114.—*Treatment of Vaginitis.* By Dr. E. J. Tilt, Consulting-surgeon to the Farringdon Dispensary and Lying-in Charity.—Whether vaginitis occurs spontaneously or as the result of uterine catarrh, it is best cured by the injection of a solution of nitrate of silver. This is an excellent idea of Dr. Jewel, but if the solution be sufficiently strong to do good it cannot be safely trusted to the patient. The patient being placed on her back, a small glass speculum should be introduced as far as possible, and an ordinary glass syringe full of a solution of nitrate of silver, containing forty grains to the ounce, should be injected. The speculum should then be withdrawn to the vicinity of the vulva, and the fluid should be left in contact for three or five minutes, after which the speculum is to be withdrawn, and the fluid received in a small cup. Sometimes I apply a speculum of appropriate size, and as I withdraw it I pretty freely touch the vagina with the solid nitrate of silver diluted by chloride of silver, as prepared by Mr. Squire. This is a modification of a plan recommended by Ricord.—*Lancet, Feb.* 23, 1861, *p.* 184.

115.—*Treatment of Follicular Inflammation of the Labia.* By Dr. E. J. Tilt.—In follicular inflammation of the labia, in eczema and prurigo pudendum, or pruritus both external and vaginal, a piece of cotton-wool should be soaked in the solution of nitrate of silver, and carefully rubbed for two or three minutes over the diseased portions of the skin and mucous membrane. I can speak with confidence of this plan, for I have lately cured several patients who had been suf-

fering in this way for four, eight, and thirty years. When cases have lasted so long the pudendal skin looks and feels like parchment. It was so in the case of a lady in whom the disease had lasted thirty years, and I first rubbed in the solution every day, then every other day, then every fourth and fifth day, until the skin became soft and pliable, and the sleep was no longer disturbed by darts of pain flashing along the nerves. This patient was cured in three months, and has had no relapse during the last year.—*Lancet, Feb.* 23, 1861, *p.* 184.

116.—*On Affections of the Hip-Joint in consequence of Uterine Disease.* (Abstract of a paper by Dr. Hoppe.)—The author reports two cases of women, one unmarried and forty-five years of age, one a widow, thirty-six years old, both of whom had been suffering for a length of time from a disease of the womb, and an affection of the hip-joint. The principal symptoms relating to the latter are :—1. Pains which occupy the whole of the thigh, especially in its anterior and exterior aspect, or only in the circumference of the trochanter : they are very intense, lasting through day and night with very short intermissions ; 2. Contraction of the adductor muscles, and of some of the flexors. Both symptoms are referable not so much to an affection of the joint itself, as to an irritation in its neighbourhood ; this becomes more likely by the fact that there exists no deformity about the nates.

As a further proof of the statement that the named affection was owing to a disease of the womb, Dr. H. remarks that he has frequently observed painful sensations around the trochanter, in connexion with uterine disease. These patients complained of a sensation of pressure and burning in the region of the trochanters, which diminished gradually, and finally disappeared under a treatment adapted to the disease of the womb. The author points to a fact, more generally known, viz., the occurrence of gonalgia as a consequence of morbid affections of the womb.—*Dublin Hospital Gazette, Dec.* 15, 1860, *p.* 375.

117.—*Large Uterine Polypus.*—At a Meeting of the Medico-Chirurgical Society of Edinburgh, Dr. Matthews Duncan exhibited a uterine polypus of very large dimensions, which he had recently removed from an unmarried woman, aged 47 years. It measured 5½ inches in length by 4½ in breadth, and was of the ordinary fibrous structure. It was, as he had pointed out in former specimens, quite destitute of muscular or mucous coverings,—being, in fact, an enucleated fibrous tumour which had been expelled from the uterine wall, its original seat, into the vagina, from which it was removed by operative interference. In this case, it was remarkable, that during all this growth, enucleation, and change of situation, the woman's health had been scarcely disturbed in the least degree. She had irregular vaginal discharges ; but as she had no suffering, she paid

little attention to them. Indeed, it was not until the supervention of a cough, which forced the tumour down upon the outlet of the pelvis, and compressed the urethra, giving rise to retention of urine, that she had any complaint to make of suffering in the hypogastric regions.

It is a mistake to suppose that the difficulty of dealing with these large polypi lies in dividing the pedicle. In the present case, the pedicle could not be reached by the finger, until the tumour was removed from the vagina by traction. This was a very laborious process, effected only by repeated and persevering efforts, assisted by the bearing down of the patient. It was, indeed, a difficult labour in a primipara; only, instead of a child's head, a relaxed vagina, and labour pains, there were a fibrous tumour, an undilatable vagina, and traction by hooks fixed in the tumour. There was no difficulty in dividing the pedicle; for it snapped across as the tumour descended, and scarcely a drop of blood issued from it. If the pedicle had not broken, it would have made little difference in the operation; for the extent to which the easily moveable uterus is dragged down in such a case, is not considerable.—*Edinb. Med. Journal, April* 1861, *p.* 954.

118.—*Chronic Mammary Abscess; Successful Treatment by Strapping, &c.* By W. Coulson, Esq.—Chronic mammary abscess, or, as it is sometimes called, encysted abscess of the breast, is a disease of considerable interest, inasmuch as it is in itself a tedious and often painful affection, and, closely simulating other more serious diseases of the same part, has many times been mistaken for tumour of the breast. A good illustrative case is now under the care of Mr. Coulson in St. Mary's Hospital.—

S. T., aged 20, single, a female servant, the history of whose affection dates back two years and a half. As is usually the case with chronic abscess at this site, its origin was unconnected with any acute lacteal inflammation, but arose without any distinct cause; it was attributed by the patient to cold. The first attack was aborted by prompt treatment. The nipple has remained retracted, and the breast subject to occasional attacks of painful swelling. A month ago, it had been unusually inflamed and painful, and presented typical features of the disorder.

The classical authority in surgery says that "the cure of these encysted abscesses of the breast may be most conveniently effected by making a puncture into them, and then passing a seton across them in a perpendicular direction; the inflammation thus excited in the tumour will speedily lead to its been softened down, and eventually disappearing." The treatment which Mr. Coulson adopted in this case, and which he commonly employs with success, was of a milder character. Scott's ointment was applied to the breast on lint; strips of plaster, of an inch and a half in breadth, were then placed tightly

around this dressing, and the whole carefully bandaged. Four days subsequently, Mr. Coulson caused the dressing to be removed and renewed. The improvement in the case was very marked, and the disease is fast progressing to a cure.—*Lancet, Nov.* 24, 1860, *p.* 510.

119.—ON THE EMPLOYMENT OF APIOL IN AMENORRHŒA AND DYSMENORRHŒA.

By M. JORET.

During the investigations into the antiperiodic power of apiol, carried on some years since by the author and M. Homolle, they had occasion to note its emmenagogue power, and subsequent trials have proved it to be not only one of the most active and most certain, but also a very harmless emmenagogue. In his preliminary observations, the author strongly insists upon the important precept that the treatment of amenorrhœa and dysmenorrhœa must be based upon an exact appreciation of the cause, thus:—1. When the cause is plethora, general or local bleeding, baths, and emollients are the best emmenagogues. 2. When it depends upon a chloro-anæmic condition, whether spontaneous or accidental, martial preparations, good diet, a healthy abode, and exercise in the open air are the means most likely to act in restoring the menstrual flux. 3. When the disturbance of menstruation is connected with general nervous excitability, prolonged cold baths should predominate in the treatment. 4. But when the cause is a deficiency of general nervous stimulus, or of that localised in the genito-uterine apparatus, a perversion in the vitality of this apparatus, and especially of the ovarian sensibility, the stimulants termed emmenagogues are indicated; and it is in these cases, which are very numerous in practice, that apiol seems to possess decided superiority to any other means. 5. In those complex cases, in which several causes seem to combine in the production of the amenorrhœa or dysmenorrhœa, we should combat the various general or local conditions already adverted to before having recourse to emmenagogues.

Not only must we first investigate the nature of the cause of the amenorrhœa or dysmenorrhœa, but also determine the exact time at which the emmenagogue should be administered. This should be always at the period at which menstruation ought to return. There may be difficulty in exactly fixing this, especially when the menses have been long absent: but there are almost always symptoms arising from a congested state of the uterus which will sufficiently fix the period for commencing the remedy. The apiol may be given in capsules, each capsule containing four grains of apiol. Generally a capsule is given morning and evening, with a little sugared water. These are continued during the four or five days of the menstrual epoch. The month after, and even the third month, if menstruation has not

become sufficiently abundant and quite regular, exactly the same procedure is to be observed. As a general rule, the menses appear more or less abundantly after the first course of doses, and it is rare for the medicine to be required to be taken after the third month. In dysmenorrhœa precisely the same course is to be observed. M. Joret quotes several cases exhibiting the decided efficacy of the apiol in primary amenorrhœa, or deficiency of secretion; in accidental amenorrhœa, or suppression, and in dysmenorrhœa.—*Bull. de Therap.—Med. Times and Gazette, Jan.* 26, 1861, *p.* 97.

120.—ON THE TREATMENT OF THE NEURALGIA OCCURRING IN CASES OF AMENORRHŒA.

By Dr. J. Y. Simpson, F.R.S.E., Professor of Medicine and Midwifery in the University of Edinburgh.

[Neuralgia is only one of many symptoms occurring in cases of amenorrhœa. Of course our treatment in such cases must be in great measure directed to the constitutional affection. One very singular symptom occurs in these cases—a "uterine asthma." The best treatment is inhalation of chloroform, or of carbonic acid. The first the patient may be taught to inspire out of her own hand; the second may be readily procured by mixing equal parts of crystallized bicarbonate of soda and tartaric acid, in a common wide-necked bottle.]

Pains under the left mamma, in the intercostal spaces, in the sacral region, or in the temples, are among the most constant and clamant symptoms seen in connexion with amenorrhœa. To relieve them you may administer some of the various narcotics—a practice, however, that may lead the patient into dangerous habits, and which, therefore, I would earnestly counsel you, as much as possible, to avoid. The use of small doses of arsenic—two drops of Fowler's solution thrice a-day will be of more ultimate benefit to your patient, although not so immediately effectual. Where there is any indication of a rheumatic taint of constitution, you will probably find more satisfactory results from the administration of thirty or forty drops of the tincture of actea racemosa, or black snake root,* three or four times daily in a little water. Locally, the application of sedative and anæsthetic liniments and plasters occasionally decreases or removes these neuralgic pains. Perhaps the most rapid relief you can give your patient will be by the subcutaneous injection of a watery solution of morphia. She will point to an undefined space, say, in the side, as the seat of pain; but if you press carefully with the finger at different spots, you will succeed in touching a point, in most cases immediately under one of the ribs, where pressure causes more acute suffering. If you adopt Dr. Wood's plan of thrusting a fine-pointed syringe through the skin

* A favourite remedy in the United States for acute and chronic rheumatism.

at this spot, and then inject ten or twenty drops of a watery solution of the bimeconate of morphia, equalling the ordinary strength of laudanum, you will in some cases succeed in relieving the pain at once; and if it does return, it will probably be in another nerve, or in another part of the same nerve, whence a repetition of the narcotic injection may possibly again expel it. Latterly, in this subcutaneous injection of morphia I have usually contented myself by sending the anodyne into any spot at the pained part without trying to search out for the purpose the supposed site of the principal branch of the neuralgic nerve. In some cases the remedy perfectly succeeds; in others it perfectly fails. And when it does fortunately succeed, the wonder is, not that it acts as a temporary local anæsthetic merely, but that occasionally the pain does not return again after the effect of the opiate has passed away. One of the first cases in which I ever tried this subcutaneous injection was in this respect one of the most successful which I have ever met with. The patient, a young Australian lady, had been suffering severely from the limited pain so often attendant upon deranged uterine health for two years. Many means, I was told, had been tried for its removal, but all in vain. I injected a few drops of a solution of bimeconate of morphia into the pained part. In five minutes the pain was gone, and has never, I believe, in any degree returned. It had much more the speed and brilliancy of a surgical than of a medical cure. But such instances are rare, very rare. And when you do use this subcutaneous injection, remember to warn your patients that sickness and vomiting may follow. Otherwise they may be alarmed by the supervention of these symptoms. The neuralgic headaches, which are sometimes so distressing to the patient in cases of amenorrhœa are usually most effectually relieved by the aid of heat, applied by means of a sponge squeezed out of hot water, or otherwise. Patients discover this for themselves; and you will sometimes find them applying mustard or turpentine to the forehead to relieve the deeper pain. In cases where the headaches have become of a chronic character, and of constant continuance, I have found the most admirable result from the use of nickel, administered in the form of the sulphate or phosphate, in doses of half a grain or a grain three times a-day.—*Med. Times and Gazette, May* 18, 1861, *p.* 515.

121.—*On the Treatment of the Eruptions occurring in Cases of Amenorrhœa.*—By Professor SIMPSON, Edinburgh.—It holds good with regard to the acne that is wont in some cases to appear each month, perhaps more distinctly than with respect to any of the other secondary symptoms arising from suppression of the menses, that the secondary disease disappears at once when the menstrual discharge is restored. But in a few cases, it still lingers on, and sometimes the disfigurement is very distressing to the patient, so that you may be

called upon to make some direct application to the affected part. The application which I have found most useful in these cases of chronic catamenial acne is the oil or butter of antimony. This is usually said to be a powerful caustic; but if you apply it to the skin, with a brush, and neutralize it speedily with some bicarbonate of soda, you will find that it causes almost no pain, and is not followed by any violent effect. What the explanation of its action may be in cases of acne I am not prepared to state; but certainly when applied in the way I have described every two or three days for a week or two, it has usually a very wonderful effect in causing the disappearance of the eruption. If after being employed for a time, it should appear to be losing its effect, it use should be interrupted, and applications should be made alternately of citrine or some other mercurial ointment, and of some of the preparations of iodine. Most of the preparations of this last drug, labour under the disadvantage of producing a discolouration of the skin, which renders their application to exposed parts always somewhat repulsive to the patient. But my friend Dr. Arkwright, of Accrington, has recently brought under my notice a preparation which seems to possess all the activity of the tincture of iodine, and which is quite colourless, so that it may be applied to the face or painted over strumous glands in the neck, without any discomfort to the patient. It is composed of one part of the ordinary compound tincture of iodine of the London Pharmacopœia, and two parts of the milder aqua ammonia. This mixture, which is at first brown, but after standing for forty or fifty hours, becomes quite clear and colourless, may be applied to the skin two or three times a-day for a lengthened period, without producing any of the pain or excoriation which attend the prolonged use of the ordinary tincture, and in obstinate cases of chronic acne it forms an admirable adjuvant to the other remedies of which I have spoken. As a local application, Rumex ointment often answers well; and if the eruption is very irritable, bismuth or cucumber ointment, with the addition of a drachm of officinal prussic acid to each ounce of these ointments. Along with these external applications, however, it is often advisable to combine the internal administration of small doses of arsenic.—*Medical Times and Gazette, May* 18, 1861, *p.* 516.

122.—ON LACTATICS.

By Dr. A. K. Gardner.

Under this title, Dr. Gardner, Midwifery Professor at the New York Medical College, designates the means capable of influencing the secretion of milk, and he arrays them under two heads—Galactics, or promoters of lactation, and Anti-Galactics, or opposers of lactation.

1. *Galactics* are again divided into galactagentia, inducers of milk, and galactagogues, expellers of milk. (1.) *Galactagentia.* (*a.*) *Liquid*

food, as milk, good nutritious soups, ale, or other malt liquors; spirituous liquors diminish rather than increase the secretion. *(b.) Fœniculum* has been lauded from the most ancient times, and is still much employed by the Germans. Dr. Gardner says that he has found great advantage attend the use of Hufeland's formula (Sem fœn., ʒj.; cort. aur., ʒss.; magn. subc., ʒiij.; sacch. alb., ʒij. M. Fiat pulv.—A teaspoonful three times a-day), the secretion having been in one of the cases suppressed for three weeks. *(c.) Ricinus communis and jatropha curcas.* Dr. Gardner does not seem himself to have used the leaves of the castor-oil plant, or of the jatropha curcas, and refers to the observations of Dr. Routh and Dr. Tyler Smith; but he says that other substances of the same class, vanilla and tonqua beans, have proved efficient in his hands. The former, when given in half-drachm or drachm doses of the extract, or when even used to flavour puddings, ices, &c., frequently exerts a marked effect, although not a very reliable one.

(2.) *Galactagogues* are articles which tend to promote the flow of the milk when secreted, relieving its retention. "Retention of milk may be caused by its becoming inspissated and thickened in its proper tubes by spasmodic stricture of the milk tubes, by acute inflammatory swelling thereof, or as the result of chronic inflammation of any number of milk-tubes near the nipple by closure of the aperture with obliteration of the canal, sometimes for an inch or more. These I have described in a former lecture, under the title of "Stricture of the Milk-Ducts and System of Sphincters or Muscular Fibres surrounding the Outer Terminus of the Reservoirs of the Milk Ducts at the Extremity of the Nipple." In this last case, medicines are insufficient, punctures of the tubes being necessary to remove the obstruction. As local applications, M. Ranques strongly recommends the following mixture:—Cherry-laurel water, ʒij.; ext. of belladonna, ℈ij.; ether, ʒj. Velpeau states that he has found this sometimes useful and frequently of no avail. He himself recommends sweet oil, ʒiij.—ʒij; ext. bellad., gr. xvss.; ammonia, ʒj.; camphor, ʒss.; yelk of egg, gr. xxx.; ether, ʒss.: to be gently applied three or four times a-day. "It is my opinion that, although so many stimulating and antispasmodic embrocations, as those of ammonia and camphor, iodide of lead, and especially mercurial friction, are strongly recommended, it is very probable that warm fomentations of water, systematic frictions towards the nipple, and systematic pressure (by means of compresses and rollers, adhesive straps, or compressed sponge) properly employed and persevered in, do in most cases all the good that can be locally done, besides natural and artificial suction."

2. *Anti-galactics.*—These agents may be sub-divided into two, Ischogalactics and Phygogalactics (1.) *Ischo-galactics*, arresters or suppressors of milk. As general means against the excessive secretion of milk, diaphoretics, mild refrigerants, arterial sedatives, saline aperients, low or moderate diet, a sparing use of condiments and

...ids, and avoidance of sexual excitement, are to be recommended. ...er and Kennedy found nauseating doses of tartar emetic valuable, ... Stillé speaks highly of full doses of castor oil. The special ischo-...otics are as follow:—*Belladonna*—A great deal of evidence has ...n published in the British and American Journals in favour of the ...acy of belladonna in drying up the milk. Still, some conflicting ...ions exist, and the author, in judging from his own experience, ...ust deduce the opinion that, while it exerts no influence upon the ...k already in the breast. it does in some cases, although not very ...arently in all, tend to diminish or suspend further secretion. A ...de of employment which I have found very effectual is smearing the ...tery extract thickly over the whole breast, and repeating the ...lication immediately after washing off the previous one, drying ...efully by compression, and removing by suction or otherwise as ...ch of the milk at the time as possible. once or twice in the twenty-...r hours; or a plaster made from a cloth or kid may be smeared ...th the extract, and may be applied partially or entirely, leaving a ...le for the nipple, so as to allow of the breast being drawn or the ...ild being applied." *Conium*, much employed by the ancients, has ...ver been tried by the author, as he thinks it a somewhat dangerous ...plication, likely to lead to atrophy of the gland. *Iodide of ...otassium* has been strongly recommended by some French and ...erman practitioners, and Dr. Gaillard Thomas says of its employ-...ent in full doses, while the breast is painted around the nipple with ...xtract of belladonna, "These two remedies have found great favour ...ith the vast majority of those who have tried them; and, although ...I have seen them fail in checking, or even in diminishing the secre-...tion, I have much oftener observed that benefit resulted from their ...use." *Sage*, used with advantage in a strong infusion by Van Swieten, ...has often since been resorted to with success. *Camphor.*—In answer to Dr. Stillé's statements that spirits of camphor is very successfully employed to suspend the secretion, Dr. Gardner says that his experience shows that camphor possesses no ischogalactic properties whatever. *Tobacco.*—"The effect of the ung. tabaci has been fully as marked as that of belladonna when employed as a plaster, as recommended for the latter. I have never seen any constitutional effects from it as is seen from the tobacco poultice upon the testicles, although the effect of belladonna, similarly used is very marked upon the iris." *Peppermint.*—"The essence of peppermint, externally applied, is with many practitioners in great favour for arresting the secretion. I have used it in many cases, and, while it failed in some, yet I am convinced of its marked efficacy. I have generally used it in the following proportions:—Ol. M. P., ℥jss.; Ol. ricin. ℥iijss.; Ol. bergamot vel jasmin, ℥jss.; camphor, ℥ij. M."

(2.) *Phygo-galactics* are articles which tend to disperse the milk accumulated in the lactiferous tubes, especially when it forms indurated tumours. Their action is supposed to be exerted on the system

of nutrition, or specially on the absorbents. They are mercurials, the tincture of iodine painted over the breast, bromine, preparations of gold and silver, belladonna, conium, camphor, and chamomile. As to belladonna, though strongly recommended by many, the author doubts whether it possesses any phygogalactic, as distinguished from its ischogalactic effect. That conium possesses phygogalactic power has been shown by D'Outrepont and others. It is possible that camphor may, as is represented by several authors, possess valuable phygogalactic properties, but the author has never derived much benefit from it, nor can he speak more decidedly in favour of chamomile.—*Med. Times and Gazette, March* 2, 1861, *p.* 231.

123.—*Glycerine and Camphor for Suppression of the Secretion of Milk.*—Dr. HARRISS having carefully tried belladonna in cases in which the suppression of the secretion of milk had become desirable, with but little success, now employs in preference a saturated solution of camphor in glycerine, gently applied over the surface of the breast by means of flannel several times a-day. The same solution is also of utility in sore nipples, although Dr. Harriss usually adds four grains of tannin to the ounce. He considers the next best means of preventing threatened abscess of the breast, is to fit the whole gland closely with adhesive plaster, leaving a hole for the nipple.—*North American Med. Chir. Rev.; Med. Times and Gazette, May* 18, 1861, *p.* 530.

124.—*Brandy and Glycerine for Excoriations and Fissures of the Nipple.* By Dr. W. FRAZER, Lecturer on Materia Medica to the Carmichael School of Medicine.—I have lately seen a mixture of brandy and glycerine in equal quantities applied with much advantage to fissures of the nipple, and from its effects I think it is worth further trials. The mixture is thick, resembling a mucilage, and may be applied as frequently as desired. Brandy itself is an old remedy for this purpose, but it soon dries up and evaporates; the glycerine has the effect of protracting its local effects, and also keeps the nipple soft and moist.—*Dublin Hospital Gazette, Dec.* 1, 1860, *p.* 365.

125.—*Sulphate of Zinc in Cancer.* By HENRY GEORGE, Esq., Kensington.—In the year 1857 a lady consulted me on account of a tumour in the right breast; it had there existed more than four years, and was evidently cancerous; her sufferings were not trifling, and her general health was somewhat impaired. In the course of a short time high authority was appealed to, which confirmed my belief in the character of the disease, and a plan of treatment was recommended. In the space of two or three months her sufferings were somewhat increased, the tumour was enlarged, and her general condition altered

for the worse, for there existed evident marks of decreased vitality. I determined to try the sulphate of zinc, a tonic whose powers I am persuaded are both safely available and very valuable. In the course of a month all pain had ceased, and her general condition was very much improved. In compliance with this lady's request I returned to the former mode of management. Scarcely a month elapsed before her sufferings returned, and her loss of strength was considerable. I persuaded her to return to the use of the zinc, and the same happy result followed. This alternation of suffering and relief from discontinuing and resuming the use of the zinc occurred three times. That medicine has now been continued for many months without interruption, and the iodide of potassium has for a long time been also administered. This lady continues to the present hour free from pain, her general health being very good, and the tumour having decreased in size at least two-thirds. Six weeks ago, a portion of the skin about the size of a shilling, of a deep red colour, took to bleeding, afterwards discharging a watery fluid; this has now perfectly healed.

The absence of pain in such a condition, the improvement of health under such circumstances, and the great decrease in the size of the tumour, are, whatever may be the result of the case, facts well worthy of consideration.—*Lancet, March* 16, 1861, *p.* 276.

126.—A SIMPLE INSTRUMENT FOR INFLATING THE LUNGS OF INFANTS.

By Dr. J. G. Wilson, Physician-Accoucheur to the Glasgow Lying-in Hospital.

[The usual methods by which the lungs of newly-born infants are inflated are all more or less objectionable and unsatisfactory. Read's double-action syringe, which Dr. Wilson has not had the opportunity of trying, was strongly recommended by the late Dr. Snow.]

The instrument essentially consists of a vulcanized india-rubber ball about the size of an orange, to which is attached a German-silver tube, about six inches long, and gently curved towards its free extremity. The tube is closed at the extreme end, but has two openings or eyes, like a female catheter, a short distance from the point. On compressing the ball, the contained air rushes along the tube and through the openings above-mentioned, and on removing the pressure the ball rapidly expands, and becomes instantly refilled with air, which may again be evacuated as before. On introducing the tube into the larynx, and acting in this manner, it is obvious that for the most part the same air would be used over and over again, which would be a manifest disadvantage and a decided objection. This, however, may be easily remedied, by making another opening in the tube, about an inch from its attachment to the ball, for the free ingress of fresh, cool, dry air. During the compression of the ball, the left thumb will easily

cover the opening, which must, however, be removed to admit the entrance of pure air during the subsequent expansion of the ball. This opening being somewhat larger than the other two, and being much nearer the ball, readily permits the entrance of fresh air. The instrument can be readily introduced in the following manner:—After throwing back the child's head a little, pass the left forefinger over upon the root of the tongue, into the rima glottidis, then with the instrument in the right hand, slide the tube along the surface of the finger (which at once depresses the tongue and serves as a guide), and on reaching the rima, insert the tube at the moment you withdraw the finger. If this plan of operating be attended to there is little fear of introducing the tube into the pharynx instead of into the larynx. It is not so necessary, as when other methods of inflation are used, to push back or depress the larynx in order to prevent the transmission of the air through the œsophagus. The insufflation of the lungs must be gently and slowly performed, so as to simulate as nearly as possible the normal respiratory process. After each inflation, the chest must be slightly compressed, in order to facilitate the expulsion of air from the lungs. The inflation of the lungs, alternated with pressure on the chest, should be steadily persevered in until respiration is regularly established, and ought never to be relinquished as hopeless whilst the least fluttering or quivering motion over the cardiac region is perceptible.

This instrument is abundantly simple, both as regards principle and construction. It can be easily introduced, and worked for any period with little exertion; and, from its small size, is very portable. The air injected into the lungs is at once pure, cool, and dry, and the force with which it may be propelled, can be easily regulated. It is not so liable to rupture or injure the pulmonary air-cells, which there is reason to fear is sometimes done in consequence of too violent restorative attempts at insufflation with the mouth or with the bellows. The irritation which the presence of the tube may occasion in the fauces and larynx, instead of being detrimental to the child, as I have heard remarked, ought of itself in many cases to have a salutary and beneficial effect, as tending to excite a gasping inspiration, or a convulsive sigh or sob. If the instrument is properly applied, as I have already remarked, the air is more certain to enter the lungs, and less likely to pass into the stomach and distend the intestines, as is too often the case when the mouth alone is used, even although the larynx be pressed back. Many infants, I am convinced, are lost from the process of respiration being impeded by the presence of fluid or mucus in the air-passages, and which, from inherent weakness and debility, they are unable to expel. This is indicated by a peculiar rattling or gurgling sound when the child attempts to breathe. This collection of mucus or fluid in the respiratory passages, is, I conceive most likely to occur when the child's head is the last part to be born, and particularly when retained in the vagina for some time. This may arise from two

causes. The impression made on the cutaneous nerves of the chest by contact with the atmospheric air induces the child to inspire, and thus to draw in mucus or other fluid lying in the vagina in contact with the mouth. The entrance of any fluid into the larynx or trachea in such cases may also be occasioned by gravitation. Of course the fluid may enter the œsophagus, but this is of minor consequence. It appears to me that this case of suspended or interrupted animation in the newly-born infant has been too much over-looked and neglected. With the instrument above described I have, on several occasions, succeeded in withdrawing considerable quantities of fluid from the air-passages, with the effect of materially relieving the breathing. When the fluid is in large quantity, the tube may be clogged up, and the ball partly filled, when it becomes necessary to withdraw the instrument, and clear it of all the fluid, otherwise it will be again injected into the air-passages. In the slighter forms of asphyxia the use of the instrument is quite unnecessary, and, when employed in the more urgent cases, it does not preclude or interfere with the use of other means, such as the alternate immersion in the hot and cold bath, affusion or aspersion with cold water, stimulants applied to the surface, or introduced per rectum, rubbing with warm flannel, galvanism, or electricity if a proper apparatus is at hand, &c.

The instrument may be obtained from Mr. W. B. Hilliard, surgical instrument maker, Renfield-street.

In those cases of suspended animation, with lividity of the child's countenance, and other symptoms indicative of cerebral congestion approaching the apoplectic condition, the respiration will frequently be much facilitated by relieving the engorged and overloaded vessels of the brain, and this may generally be done by allowing a drachm or two of blood to escape from the divided vessels of the cord before applying the ligature. When much difficulty is experienced in the birth of the child's head after the delivery of its body, the salvation of the infant may I am convinced, in many cases be secured by establishing intra-vaginal respiration. There can be no doubt but that, in the absence of the forceps in such cases, a degree of traction on the neck of the child is sometimes exerted before the head is delivered which is of itself almost incompatible with life. Indeed, in two or three cases which have lately come under my own observation, the child, although so far resuscitated as to respire by its own efforts for several hours, yet the injury done to the cervical region was such as to preclude all chance of its ultimate survival. The application of the forceps in such cases does away with the necessity of dragging or pulling much upon the neck; but these instruments are unfortunately often not at hand when required, and some time must necessarily elapse before they can be procured, thus occasioning a delay which will in all likelihood be fatal to the child if let alone. As soon then as it is ascertained during the course of labour that the head of the child will be the last part of it born—especially if any difficulty or delay is anticipated in its transit—the forceps should be sent for, and kept in readiness in the event of

their being required. When the head is firmly retained after the birth of the body, and the forceps are not at hand, endeavour, if possible, to excite and maintain the respiratory process till they be procured. This may sometimes be accomplished by merely keeping the child's mouth open with the finger, and the perineum retracted, so as to permit the free access of air into the vagina, or the instrument above described—either as it is, or with an elastic catheter, or a flexible tube substituted for the metallic one—may perhaps be employed with greater prospect of success.

The infant at birth clings with such tenacity to life, that—in the absence of putrefaction, or any malformation, defect, &c., incompatible with life—it becomes often a very difficult matter to determine with any degree of certainty whether the vital principle is actually extinct, or animation merely suspended. We should, at all events, never too hastily despair of effecting the complete restoration of the infant, however unpromising the case may at first sight appear. There is good reason for believing that many infants, especially amongst the lower classes, are annually consigned to the tomb, whose lives might have been preserved had resuscitative means been fully and fairly tried.—*London Med. Review, Dec.* 1860, *p.* 299.

127.—*Treatment of Prolapsus Ani of Children by Subcutaneous Injection of Sulphate of Strychnia.* By M. Foucher.—A child, 4 years old, came under the care of M. Foucher, who had suffered for several months from prolapsus of the mucous membrane of the rectum. When not returned immediately, great pain and difficulty attended its reduction, owing to its being closely grasped by the sphincter. M. Foucher determined to apply Wood's plan of subcutaneous injection to this affection, in order to act directly upon the fibres of the sphincter. He therefore, with one of Pravaz's syringes, injected in the direction of its fibres, and just external to the anus, ten drops of a solution of sulphate of strychnia, in the strength of twenty centigrammes to twenty grammes of water. Twenty-four hours afterwards fourteen drops were again injected. The cure was immediate, and six months afterwards had remained durable. Without attaching undue importance to the cure of a case of disease which is sometimes very easily relieved, it is deemed desirable to direct attention to a mode of treatment so simple, and accompanied by so little pain.—*Gazette des Hôp.; Med. Times and Gazette, Nov.* 10, 1860, *p.* 463.

128.—POLYPUS OF THE RECTUM IN A CHILD, SUCCESSFULLY REMOVED.

Case under the care of Thomas Bryant, Esq., at Guy's Hospital.

Polypus of the rectum, although recognised by surgeons as a distinct disease, has not received from them the attention which it deserves; and children, labouring under this affection are too often,

from the want of a due appreciation of the subject, allowed to go on for many months losing blood from the anus, and tortured with the irritation of a prolapsed bowel. A good illustration of this fact, and of the disease, has been brought to our notice under the care of Mr. Thomas Bryant, who, it may be remembered, brought the subject before the profession by an interesting paper published in the Lancet in Nov. 1859. (See *Retrospect*, vol. xli., p. 334.)

The case was that of a boy two years and a half old, who for upwards of nine months had been daily losing blood from the bowel; at times this hemorrhage taking place during defecation, but occasionally at other periods. The mother had observed the whole bowel to be now and then prolapsed, and intense straining and pain to accompany each motion. When the bowel had been returned, a nut-like growth would often remain, which was with difficulty reduced. The child had been taken to many surgeons, who had treated it with medicines, but none had ever made a local examination. The character of the disease was readily recognised upon a digital examination, a polypus of the size of a large nut being detected, fixed by a thick pedicle to the bowel about an inch and a half within the sphincter. Mr. Bryant with his finger hooked the growth down, and was about to put a ligature on the pedicle, when a sudden action of the sphincter and levator ani muscles took place, breaking off the growth, and thus removing the disease. All symptoms immediately ceased, and a good recovery took place.

Polypus of the rectum may, however, be found in the adult, although it is not so common as in children, according to Mr. Bryant's experience. The following is a good example lately under the care of that gentleman:—

A man, aged 66, for six weeks had observed that during defecation something protruded from the anus, which was with difficulty reduced, bleeding generally accompanying its descent. For twenty-four hours previous to his application to Mr. Bryant this growth had remained prolapsed, and as he had been unable to return it, he sought advice. Upon examination, a firm fibrous polypus, of the size of an almond, was detected growing from within the sphincter. A ligature was applied to its pedicle, and on the second day the growth fell off, recovery taking place.

We would remark, that the occurrence of polypus of the rectum in the adult is very rare, as pointed out by Mr. Curling in his work on "Diseases of the Rectum." He states that it generally occurs in early life, and that in children it usually makes its appearance external to the anus after a stool, the growth resembling a small strawberry, being of a soft texture, granular on its surface, and of a red colour. It has a narrow pedicle, of variable length; it produces no suffering, but requires to be replaced with the fingers when protruded, and causes a very slight bloody discharge, which excites some alarm. The description of the complaint given by the mother or nurse is liable to

mislead the practitioner into the belief that it is simply prolapsus. The nature of the disease can be determined only by an examination of the tumour on protrusion, when the treatment will be obvious enough.

[These polypi are not so rare as is here expressed. The rarity we believe consists in not detecting, or even suspecting them: at any rate they are not so very uncommon in females. The reader will find some good illustrations of this remark in our last volume by Mr. I. Baker Brown, (vol. xlii, p. 349), who finds that many fissures in the anus in females have each a polypus hanging or pushing into the fissure from the opposite side of the rectum, which cases are easily cured by ligaturing the polypus and dividing the fissure with Copeland's blunt-pointed straight bistoury. To divide the fissure only would not suffice.]—*Lancet, Oct.* 27, 1860, *p.* 410.

129.—ERRORS RESPECTING DENTITION.

By Dr. JACOBI.

Introductory to his lectures on "Dentition and its Derangements," Dr. Jacobi makes the following observations:—

"You know that, among the public at large, even among the educated portion of the community, teething is regarded as one of the two scape-goats of all diseases of infantile age. Teething and worms are among matters acknowledged as the universal and all-powerful sources of disease. Whenever an innocent ascaris or puny oxyasis is observed in the fæces of a child, worms are, for years to come, considered as the undoubted cause of any disease that may occur. Teething, a normal, physiological development, taking place at an age which, for many reasons, is subject to a large number of diseases, has a strong hold on frightened maternal minds. The first dentition generally occupies the first two years of infantile life, a period in which the child is peculiarly liable to diseases both numerous and frequently dangerous. As the protrusion of a tooth (and on the average, a tooth will cut every month) is a remarkable phenomenon, and is something new and visible, it is believed to be the cause of every unfavourable occurrence in early life. A mother will bring to you her child, thin, emaciated, and anæmic, with sunken eyes and the wrinkled physiognomy of old age, and tell you that she is well aware the poor thing is suffering from teething, and that, therefore, nothing can be done to alleviate its sufferings. She will never be convinced that the child is dying from her own neglect; but she has allowed a slight catarrh of the intestines, perhaps, to degenerate into incurable follicular ulceration. . . . . Teething is thus considered the most efficient cause of most of the terrible diseases which prove fatal to thousands of the rising generation. I can assure you that the readiness to attribute all the diseases of infantile life to teething has

destroyed more human beings than many of the wars described in history. For, though parents are so much impressed with the belief of the dangers of teething, still they never think of attempting to save the lives of their children by counteracting the supposed life-endangering power of a normal process.

"What is now the belief of the public has been the conviction of the medical world through centuries down to the present time. General experience shows that the persuasion of the scientific world, after having been given up to make room for more correct opinions, has remained in the public at large, and it is to be feared that it will not soon be removed. And it would be fortunate if the prejudice were confined to the public. But, unfortunately, it still lingers in the medical profession, and it is for this reason that I have dwelt upon it thus lengthily. Nothing is more common than to hear doctors, young and old, in cases of infantile disease, diagnosticate 'teething,' after mother and nurse have done so before; and nothing is more frequent than to be told that the death of a child was the consequence of dentition. I have seen in this city (New York) a certificate of death in which the direct cause of the death of a child five years of age, with his jaws full of teeth, was stated to be 'teething.' Consider for a moment the absurdity of the conclusion, that a normal, physiological process is fatal to the existence of a living being! Who has ever ventured to assert that menstruation, pregnancy, or the climacteric years are the direct causes of death? It is equally absurd to assert it of dentition; and yet such statements are daily made by physicians. According to the Census of England for 1857, there were in the United Kingdom (England alone) 3992 deaths from teething, 3791 of which occurred in children of less than two years, and 201 in children from two to five. Between 1845 and 1850, no less than 3466 infants are reported to have died in London from teething, and the disorders caused by the general irritation attending dentition —the number of the deaths from all causes being 258,271, giving the proportion of 1 death from teething to 74 from all causes. In the State of New York there died 626 children from teething in 1855; but it is not stated whether a part of these unfortunate children had not the full contingent of teeth of the first dentition."

After adverting to the graver symptoms sometimes met with during dentition, Dr. Jacobi continues:—"In a certain number vomiting will stop, but the diarrhœa continues. The deluded mother, who felt a little uneasy at the severe character which teething seemed to have assumed, is gratified, after the main symptoms have passed by, to find that her child is suffering from diarrhœa only, and that, in this manner, teething will be made easy and comfortable. But, alas! this deception on the part of the mother is too often fatal to the child. The diarrhœa is allowed to go on for days and weary weeks; the digestion becomes hopelessly destroyed, the abdomen immensely distended with gas, the mesenteric glands swollen and impermeable to

chyme, the catarrh and over-secretion of the glandular follicles of the intestine lead to deep ulcerations of the intestinal canal, the diarrhœa becomes also more frequent, serous, mucous, or bloody, the arms and legs of the little sufferer dwindle away, and the countenance becomes emaciated and senile. The scene closes with a consoling certificate from some doctor or druggist, affirming that teething was the cause of death. Thus 'millions' of infants are destroyed by ignorant, prejudiced, and incorrigible advisers. I say incorrigible. I know that mothers will always consult their prejudices first, the prejudices of their neighbours next, perhaps, at some later time, common sense, and finally they may seek the advice of an educated medical man. I know that a mother who has consigned a beloved child to the grave, will repeat the follies which cost her the child she has lost. If you remonstrate with her for neglecting the second as she did the first, she will reply, Was not the child teething? Would you prevent it teething naturally? Was it her fault that the child got teeth with difficulty? The true inference would be, that nature neglected much, and that it was greatly at fault in the matter of dentition. I once read a newspaper announcement of the death of a child, in which the parents affirmed that 'the Lord had hauled the dear child up to heaven by the teeth.' Now, in this case, neither the father nor the mother was at fault. . . . . It will better answer my design to give you a sketch of what dentition is anatomically and physiologically, in order to show clearly the normal and abnormal course it may take. I shall thus be able to explain and limit the numberless complaints generally attributed to it. If I can relieve your minds of the impression, that dentition destroys thousands, and even tens of thousands, of innocent beings, who are yearly sacrificed, in reality, to the prejudices of other times, I shall be abundantly satisfied."—*American Med. Times; Med. Times and Gazette, Dec.* 29, 1860, *p.* 643.

MISCELLANEOUS SUBJECTS.

130.—REMARKS ON A CASE OF POISONING BY STRYCHNIA—RECOVERY.

By Dr. JAMES PART.

I was called on the forenoon of the 1st of October, 1860, to see Eliza M., aged 31 years, a domestic servant, who was reported to have taken poison. About a quarter of an hour before I arrived, she had been found on the floor of the kitchen by her mistress, whom her screams had brought to her assistance, and when an attempt had been made to lift her up to place her on a chair, she had been instantly seized with twitchings, followed by powerful spasms of the limbs and trunk, with loud and piercing screams.

When I first saw her she was recovering from this paroxysm. She was sitting on a chair, with both legs stretched out, the feet being widely apart and everted, and the toes flexed; the arms were also powerfully extended, and the hands convulsively grasping the arms of two men who were supporting her on the chair, from which she appeared very much afraid of falling. The body was in a state of tetanic spasm, being powerfully extended, or indeed bent somewhat backwards, approaching a state of opisthotonos, and rested entirely against the top of the chair-back and the front edge of the seat. The expression of the countenance was wild and anxious; the face blue, very like a person suffering from cholera, and covered with a cold perspiration; the eyes staring and prominent; the breathing irregular and convulsive; the pulse was so quick that it could not be counted, and had a peculiar fleeting, indistinct character.

The patient was muttering indistinctly some prayers for forgiveness of her crime. On laying my hand on her shoulder, as I asked her what she had taken, the muscles of the trunk and limbs were thrown into a state of spasmodic contraction, and this repeatedly took place when any part of the body was touched; even the application to the lips of a glass of water produced these phenomena to a considerable degree. From these symptoms it was evident to me that she had taken strychnia in some shape, but either from unwillingness to tell what she had taken, or from the confused state of her mind at the time, I could not then ascertain in what form. I saw her at half-past ten, and the poison had been taken about a quarter before ten; consequently about three quarters of an hour had elapsed, during which time the poison had been affecting her.

I immediately administered a strong emetic of sulphate of zinc and ipecacuanha, and sponged the head with cold water, though I must confess with little hope of a successful result, being apprehensive that absorption of the poison had taken place in sufficient quantity to destroy life. As the emetic did not act for nearly three quarters of an hour, I had to consider the propriety of applying the stomach-pump; for during that period she continued to have slight tetanic spasms resembling electric shocks, which followed any movement or any touch of the body. She all the while manifested great fear of falling, would not quit her hold on the hands of those about her, and continually ejaculated "Oh, hold me!" The legs were continually drawn asunder involuntarily, and the position appeared painful to her, as she repeatedly desired to have them placed together. I was deterred from using the stomach-pump by considering the effects produced by the application of the glass to the mouth, by touching the body, or by tickling the throat with a feather to excite vomiting, all of which brought on the tetanic spasms, the latter somewhat severely.

A second emetic was therefore administered, and cold water continued to be applied to the head. After a while she became very much exhausted, the spasms were more frequent, and she begged to be laid on the floor, having lost the use of her legs. This movement brought on the most alarming attack of spasm it has ever been my lot to witness, except perhaps one, which proved fatal, in a case of traumatic tetanus. The arms were powerfully extended; the legs also extended, and drawn widely apart; the muscles of the back were in a state of the most powerful contraction—in fact, of complete opisthotonos; the expression of the countenance was wild; the face had a dusky, purplish hue; the eyes were wide open, the eyeballs protruding, and the pupils so widely dilated that the irides were barely visible. After remaining some seconds in this condition, the whole muscular system appeared at once to quiver, the spasms relaxed, her hands loosed their hold, the arms falling by her side, the jaw dropped, the angles of the mouth were drawn down, she heaved a deep sigh, and I believed her to be dead. After some seconds, another inspiration, brought on apparently by cold affusion to the head and chest, seemed slightly to restore animation, and she gradually began to breathe—very imperfectly at first, but afterwards with some regularity; and at length she recovered from the attack. In about ten minutes after this she began to vomit, to facilitate which copious draughts of warm water were given. She continued to vomit at intervals for more than two hours. As the vomiting continued, so the spasms appeared to abate in their intensity; and in about four hours from my first seeing her we were enabled to get her up-stairs to bed without the occurrence of the spasms, which, previous to the last severe fit, had come on upon every attempt to move her from the chair. As soon as the sickness had in some degree abated, a mixture of extract of Indian hemp with chloric ether was administered, with a view of diminishing

the spasms and counteracting the prostration she evinced; but most of the mixture was quickly rejected by the stomach.

On my next visit the spasms had subsided into slight occasional tremors, and the skin was becoming hot and the pulse fuller; reaction appeared fairly to have set in. An effervescing mixture was therefore prescribed, and, to clear out the bowels, a castor oil draught. At nine o'clock at night—eleven hours after she had swallowed the poison—she appeared tolerably well and free from pain, except some soreness in the muscles of the arms and legs. On the following morning she appeared as well as if nothing had happened. There was, however, some slight heat of skin, and the tongue was somewhat furred. As the castor oil had not acted, a dose of calomel and colocynth was given, followed by an aperient draught; the saline mixture was continued. The next day—forty-eight hours after she had taken the strychnia—she was quite well, and requested to be allowed to go home to her relatives. She has twice since called upon me, and states that she is quite well; she expresses great contrition for her offence.

In one of the intervals between the attacks of vomiting, the patient pulled out of her pocket the paper and envelope which she said had contained the poison, and which proved to have been a packet of "Battle's vermin-killer," the active agent of which is now too well known to be strychnia, from the numerous instances of fatal poisoning which that substance has caused. The poison had been procured on the preceding evening from the shop of a neighbouring druggist.

I am informed by my friend Dr. Letheby, who has had opportunity to examine this substance, that the poison contains about twenty-three per cent. of strychnia; and as each powder weighs, pretty uniformly, about fifteen grains, we may assume that three grains of strychnia had been taken in this case. Dr. Letheby was kind enough to analyse a portion of the liquid contents of the stomach for me, and obtained the most unequivocal evidence of the presence of strychnia, which my own more clumsy manipulation also obtained satisfactorily.

Having now given a concise, and as nearly as I am able, a correct account of the more prominent features of this case, as I observed them, I am desirous of making a few observations, suggested by contemplating its various features, on the means which proved successful in my hands. Being few and simple, we shall easily arrive at a correct conclusion as to which the recovery may be fairly attributed; and, I am inclined to attach much importance to simplicity in our remedial measures, as being the only way of arriving at accurate notions on the subject of treatment. But before I make these observations, I will endeavour to bring under notice the principal facts connected with the treatment of those cases of poisoning by strychnia which have recovered.

Several substances have been brought forward, both by physiologists and practitioners, to which the designation of antidotes has

been given, by which I understand remedies possessing the power of neutralizing injurious effects on the animal economy. These would, doubtless, first be present to the minds of gentlemen on finding themselves face to face with a case of this description. But we must bear in mind that these cases are not often observed until the effects of the poison are manifest, and when of course a deleterious, perhaps fatal, quantity must have found its way into the circulation. An antidote can, therefore, affect only what remains unabsorbed in the stomach; it cannot possibly interfere with that which is already circulating through, and exerting its baneful effects on, the system. When we consider that death usually takes place in less than two hours after the poison has been swallowed, we are reminded that no time is to be lost in the application of our remedies, when dealing with an agent,

> "Whose effect
> Holds such an enmity with blood of man
> That, swift as quicksilver, it courses through
> The natural gates and alleys of the body."

The antidotes that have been brought forward are somewhat numerous—namely, tobacco, and its active principle nicotine; woorari poison; chloroform; hydrocyanic acid; camphor; animal charcoal; tannin; tincture of iodine; and lard or fat; nine in all. The first four of these have their pretensions founded upon a physiological probability, based on the fact of their actions being antagonistic to the visible effects of strychnia on the system. The first, nicotine, was introduced to the notice of the profession by the Rev. Richard Haughton, of Trinity College, Dublin, in a paper read before the Royal Irish Society. Reasoning upon the known properties of strychnia, as compared with those of tobacco, to which they are diametrically opposed,—the former being a very powerful excitor, and the latter as powerful a depressor, of muscular contractions,—he came to the logical conclusion that tobacco ought to be the natural and appropriate antidote to strychnia. In order to prove the truth of his theory, he immersed two frogs in solution of nicotine, which caused complete prostration or paralysis of motion, and the animals died paralysed in twenty-three minutes. Others he placed in solutions of strychnia, which produced the usual tetanic effects of that drug, and caused death in *four* minutes. He then placed two others in a compound solution of the two poisons. The first frog lived in the compound solution forty-seven minutes, at the end of which time it died. The second frog was kept in the solution ten minutes, and then taken out; but after its removal from the liquid, in thirty minutes it was seized with the tetanic spasms of strychnia, in the form of emprosthotonos, but ultimately recovered.

We thus learn from these experiments that for a time the effects of the strychnia were suspended by the nicotine; but that in one case the fatal result was not prevented, and in the other the influence of the strychnia was only deferred. But I do not consider these experi-

ments calculated fairly to test the power of nicotine or tobacco asan antidote, inasmuch as the remedy was given at the same time as the poison, whereas, to arrive at an accurate result, the experiment ought to approximate as nearly as possible to the realities of a case—that is to say, the antidote should not be given until the manifestations of poisonous action are apparent. The only case on record of the results of this agent is one by Dr. Byrne, of St. Louis, Missouri, who, having read the account of Mr. Haughton's experiments, administered an infusion of tobacco, that being ready at hand. This man, who had taken an emetic before Dr. Byrne saw him, and which had acted freely, recovered in twelve hours.

In support of the claims of woorali poison as an antidote for strychnia, we have some experiments of Dr. Harley, of University College, which are very conclusive of the neutralizing effects of these drugs one to the other. In the first experiment, a frog was poisoned with the 500th part of a grain of woorali; three minutes after he had become perfectly insensible, the 120th part of a grain was injected; in five minutes he became tetanic. The second experiment is the converse of the first. A frog was poisoned with 120th part of a grain of strychnia; in three minutes after tetanus was strongly marked, he was punctured with a 500th part of a grain of woorali; in seven minutes the tetanus disappeared. There are yet no recorded cases of strychnia poisoning in the human subject in which this remedy has been used. It has, however, been applied endermically in three cases of traumatic tetanus by M. Vella, in the French Military Hospital at Turin. In the first two, although the patients died, their sufferings were very much alleviated by the muscular relaxation produced by the woorali; the third case recovered under its use. Mr. Spencer Wells has recently also published an account of three cases of tetanus after ovariotomy, in which he used this remedy. His results were precisely similar to those obtained by M. Vella—two died, and one recovered. Its effects being exerted entirely on the voluntary muscles, give it a sort of parallelism to strychnia. To my mind there is a very valid objection to its use, in the fact that its preparation by uneducated savages must be very uncertain, and that when given by the stomach it is at times almost inert, and always uncertain in its operation; and the observation of Dr. Taylor, that "the patient is likely to be killed by the so-called antidote if he escapes from the poison," stands in great risk of being realized in reference to woorali.

The physiological claims of *chloroform* to be ranked as an antidote are not quite so great as those of tobacco and woorali; it is, however, better known to us, and has the *prestige* of having been administered in four cases of poisoning by strychnia, and with good results. The first is noticed in the Medical Gazette for August 2nd, 1850. An intemperate man, aged 40, had swallowed about one or two grains of strychnia. The usual remedies having proved unavailing, chloroform was administered by Dr. Munson, of Boston, U.S. One drachm was

inhaled on a silk handkerchief, and we are told "the effect was decisive." Previous to inhaling the chloroform, he could not be moved without the spasms coming on; but immediately afterwards he requested to be laid in the recumbent posture, which did not now excite the spasms. The chloroform was carefully administered for some hours, and the patient rapidly recovered.

The second case is by Dr. Jewitt, of Boston, who states that the patient, a boy about fifteen years old, was kept in a state of partial anæsthesia for four hours and a half, and that as often as the chloroform was withdrawn, the spasms came on, but were immediately subdued by it. The boy recovered well.

The third instance is mentioned in the Pharmaceutical Journal for January, 1857, and is taken from a West Canada newspaper. A city police-officer swallowed, by mistake, four grains of strychnia. Finding out his error, he quickly swallowed an emetic; he subsequently took a second, and a third was administered by Dr. Bly, and vomiting was kept up by warm water. Chloroform was administered during seven hours, after which the spasms ceased.

Another, being the fourth case, was a young woman, who had intentionally taken about two grains of strychnia enclosed in a sweetmeat. After she had begun to feel the spasms come on she repented, and took an emetic of common salt, which caused her to vomit. When brought into University College Hospital, Dr. Hillier, by whom the case is reported, applied the stomach-pump, and chloroform was inhaled to allay the spasms. The symptoms were not severe, and she soon recovered.

The next case I have to notice was cured by the exhibition of chloroform by the stomach. A man was seen by Dr. Dresbach, of Ohio, in twenty minutes after having taken three grains of strychnia. Dr. Dresbach gave him two drachms of chloroform, and in less than fifteen minutes after swallowing it, the relief is said to have been complete, and he quickly recovered.

The greatest number of recoveries from poisoning by strychnia are those in which *camphor* has been exhibited. There are five in number. The first I find is reported by Dr. Pidduck, physician to the Bloomsbury Dispensary. A dissolute man, a pianoforte maker, of weakly constitution, had been ordered strychnia, in doses of one-sixteenth part of a grain for some neuralgic affection. Owing to an error in compounding, the medicine was prepared in doses of one-sixth instead of one-sixteenth. The first dose produced some twitchings of the muscles. A second was, however, taken, which produced the usual severe spasms from poisonous doses of this drug. Dr. Pidduck found him in great danger, and prescribed five-grain doses of camphor, suspended in almond emulsion, which at once stopped the spasms. The medicine was continued throughout the night, and the man recovered. Two cases are mentioned in the British and Foreign Medico-Chirurgical Review for January, 1857, as having happened in the practice of Dr.

Rochester, and communicated to the Buffalo Medical Association, but in one of these cases half a grain of morphia was given. In the same number of the Review, Dr. Richardson, in the report on Forensic Medicine, alludes to the case of a gentleman who had taken two ounces of laudanum, and subsequently two pills containing ten grains of strychnia. Dr. Givens, by whom the case was reported, found him vomiting freely, so that he must have taken an emetic also. Two large doses of tartaric acid were given, followed by camphor mixture, alternating with doses of ether and turpentine. Amidst this variety of remedies the patient recovered. A successful case is also reported in the Lancet for April 25th, 1857, by Dr. Pritchard, of Filey, Yorkshire, in which three to five-grain lumps of camphor were successfully administered. It must be observed, that in this case emetics were given, and the stomach-pump used, with Dr. Marshall Hall's ready method of artificial respiration.

Animal charcoal has been brought forward as an antidote by Dr. Garrod, who found it exert a preventive influence on the action of strychnia, as well as on other poisons, in some experiments which he performed on animals. A case in which it was given was related by Mr. W. Chippendale to the Abernethian Society. A strong man took four grains of strychnia, with an equal quantity of morphia. In rather more than half an hour violent spasms came on, and when admitted into St. Bartholomew's Hospital he had lost the use of both his legs. Three or four ounces of animal charcoal, suspended in water, were thrown into the stomach, which was subsequently emptied by the stomach-pump. He continued very ill for some time afterwards; the paroxysms were frequent and violent; pulse 150. Chloric ether and brandy were given, and he rapidly improved; the convulsions ceased, he passed a good night, awoke free from convulsions, and was discharged next day.

Of *hydrocyanic acid* as an antidote we have only the evidence of one case, and that not in the human subject. It is reported by Dr. Bewley, who, wishing to poison a mangy cur, gave it half a grain of strychnia. The sufferings of the animal were so severe that he could not bear to witness them, and, in order to put a speedy termination to them, he gave the dog half a drachm of prussic acid mixed with a little milk, which the poor brute eagerly lapped up. Instead of falling down paralysed, as the doctor had expected, the dog got upon his legs, ran away, and recovered.

Tannin is the next remedy I shall mention. It has been very recently put forth by Dr. Kurzak, in the Art Journal of Vienna, as the best remedy in poisoning by strychnia, if promptly administered. The recommendation is based upon experiments on animals, but the particulars are not given. Dr. Kurzak, however, was not the first to recommend this agent. In the British and Foreign Medical Review for July, 1842, a case of recovery from poisoning by strychnia is reported by Dr. Sudicke, in which half a grain of strychnia had been taken in

six hours. The symptoms became very alarming. Dr. Sudicke ordered ice to be applied to the head, and half a grain of tannic acid every half hour. Twelve grains were taken and followed by decoction of oak bark. Meaner, of Dresden, also recommends decoction of oak bark or galls.

We come now to another remedy, *tincture of iodine*, recommended by Dr. Bennett, of Sidney, N.S.W., who has recorded a case of recovery from poisoning by a paper of Battle's vermin-killer, in which recovery is attributed to the use of this remedy, but upon what grounds does not appear. It is worthy of remark that the lady had taken several scruple doses of sulphate of zinc previously, which acted freely, and the convulsions ceased in twenty minutes after she had vomited.

Lard has also been recommended as an antidote by Dr. Pindell, but its efficacy is disputed by Dr. Hammond, of Fort Riley, Kansas, who, in a letter to the editor of the American Journal of Medical Science, says: "I have tried Dr. Pindell's antidote to strychnia—lard. I gave two grains of the poison to one dog without the antidote, and two grains to another with the addition of a pint and a half of melted lard. The best of the joke is, that the latter died in four hours, and the former—a miserable, worthless cur, who doubtless was too mean to die—is still running about in the finest possible state of health. So much for lard."

Emetics.—I now proceed to examine the claims of emetics to effect our object—or rather I would say vomiting; and to prove that this is the agent I will draw attention to its effects in some diseases manifesting phenomena not very dissimilar from those caused by the poison under consideration. I allude to those disorders having a paroxysmal or spasmodic character—namely, whooping-cough, hysteria, and infantile convulsions. In all these, in common with strychnia poisoning, we have, first, the periodical return of the paroxysm or spasm; secondly, the intermittent and irregular performance of the function of respiration; and, thirdly, a dark and highly carbonized condition of the blood. My attention was many years ago directed to a fact which is no doubt familiar to the profession, that the paroxysm of whooping-cough is almost invariably cut short by a good fit of vomiting, and that the countenance, which before was dusky from the circulation of highly carbonized blood, soon becomes florid and animated; the respiration, which before the fit was laboured and irregular, now becomes free and easy, and the child returns to its play or its meals. In the convulsions of children we have the same irregular, intermittent, and imperfect performance of the function of respiration, and the same irregular distribution, generally diminution of animal heat.

Reflecting on the results of vomiting on the paroxysm of whooping-cough, I was induced to try it in convulsions, and, where I could obtain it, have never found it fail in removing the convulsion.

In the allied disease of hysteria, where a very similar condition of the respiratory organs and functions exists, the breathing here being

irregular, imperfect, and often at distant intervals, and where the external manifestations very much resemble those we have been considering, I have for many years looks upon vomiting as the remedy above all others to be relied on to remove the paroxysm or fit.

In the cases above mentioned it would be unfair to regard the mere emptying of the stomach as the *ratio medendi*. I have long since come to the conclusion that the return to healthy respiration has more to do with the relief of these diseases than is generally admitted. I believe the experiments of Dr. Harley have led him to the conclusion that strychnia produces death, not by its action on the nerve substance, but on the blood, by destroying its capability to absorb oxygen. This theory appears to derive considerable strength from the fact of the blue or dusky condition of the skin in the subjects of poisoning by strychnia, indicating a highly carbonised state of the blood. It has been suggested by Dr. Brown-Séquard, that in many cases of poisoning the loss of animal heat tends to a fatal result. Now, what is the meaning of this fact but that it is in consequence of the respiration being imperfectly and insufficiently performed, and the circulation through the lungs having been thereby impeded, that the blood is not passing with its accustomed energy through the capillaries, and this tendency to death takes place? A similar condition to this must exist in the cold stage of fevers; and here we give emetics—and what for, if not to bring about, by the action of vomiting, a more complete and full respiratory action, and a more perfect series of that action, by which the blood is more quickly driven through the lungs, causing an increased absorption of oxygen, and a consequent greater development of animal heat, with its more general diffusion over the body? These results of vomiting, which are self-evident, cannot be the effect of the mere evacuation of the contents of the stomach; they must therefore be accomplished by the physiological action of vomiting. If this be true, then, *we have, in vomiting, a real and true antidote to poisoning by strychnia; and it is by the maintenance of the condition of system thereby induced that we must reasonably hope to combat the two morbid effects of the poison which are said to tend to death—I mean the diminished capacity of the blood for oxygen, and the consequent falling of the animal heat.*

But although I believe we have sufficient evidence to prove that this alone, when freely accomplished, is of itself sufficient to prevent death, yet I would not limit the treatment entirely to its production; for after the evidence we have had of the power of chloroform to subdue the spasms, I think we shall consult the comfort and ease of our patients by its careful exhibition, but I do not think it should be carried to the pitch of producing complete insensibility, nor, indeed, beyond the degree which causes the breathing to become regular. When the sickness has somewhat subsided, we may give camphor, suspended in almond emulsion, or any other fitting vehicle. With a view of assisting to maintain the animal warmth, I would place the patient near a good fire, or keep him well covered with blankets. It

is of the utmost importance that the most perfect quiet should be observed by the patient and those about him, since it is obvious that any touch of the surface, or movement of any attendant in contact with the patient, may bring on a paroxysm of tetanic spasm, which might prove fatal. During the very severe fit, I believe we derived most timely aid from cold affusion, which appeared to bring an inspiration, and restore the then suspended animation. These remedies, though useful auxiliaries, are quite subordinate to the more important act of vomiting.

When once vomiting has been brought on, we should assist it by copious draughts of warm water, which will prolong the sickness, and also render it less painful. As regards the kind of emetic to be used, of course we give the preference to those of a non-depressing character; and were I to see another case, I should be disposed to use mustard, as being quicker in its operation than all others. Sulphate of zinc and ipecacuanha, however, afford a very appropriate form.—*Lancet, March* 30 *and April* 6, 1861, *pp.* 311, 335.

131.—*Nitrate of Ammonia as a Tonic.* By Dr. WALTER COLES. —The attention of the profession has been recently called to the nitrate of ammonia as a means of introducing an increased quantity of oxygen into the system. A female is now under treatment in the second surgical division, in whom the nitrate of ammonia has been used with the most satisfactory results. She had suffered for a long time from disease of the knee-joint, when the limb was amputated in the thigh by Dr. A. B. Mott, in the early part of August. For the first four days after the operation the patient was in a most precarious state, and apparently liable to sink at any moment. She was freely stimulated with brandy and milk punch; but still she continued to decline. Suppuration from the stump was most profuse, and it seemed to be making little or no progress towards reparation. Quinine, carbonate of ammonia, and chlorate of potash, were each in turn tried with no better success. At the suggestion of Dr. Barker, they were now stopped, and nitrate of ammonia, grs. xv., three times a day, administered, while the brandy and ale were continued in small quantities. From the moment the nitrate of ammonia was begun, the stump assumed a more healthy appearance, and her general health began to improve. She expressed herself in the most decided terms in regard to the relief derived from it. The amendment has been gradual but steady ever since.—*American Med. Times, Nov.* 3, 1860, *p.* 311.

132.—A NEW SALT OF IRON AND QUININE. By Dr. WALTER FERGUS, Lecturer on Chemistry at Marlborough College.

[The author states that he has used the following combination of iron and quinine for many years with very satisfactory results. He says:]

Iron and quinine are already represented in so many forms that the addition of a new one may be looked upon as an unnecessary undertaking. And it is most true of medicines in general, that a multiplicity of forms is a great evil; but there are certain points in this new combination which seem to me worthy of attention.

It is generally found that a salt of the protoxide of iron is preferable to one of a higher degree of oxygenation: but it is also difficult to obtain an absolutely permanent salt of the protoxide. Perhaps, without exception, the sulphate is the most practically useful of all the salts of iron, owing to the uniformity of its composition. Of the quinine salts, the sulphate is also the most available for general purposes. It is not difficult to form a simple combination of these two sulphates, but the resulting compound is not well fitted for general use. The addition, however, of a certain proportion of sulphate of magnesia, enables us to obtain a salt which is nearly as soluble as the sulphate of magnesia itself—quite unalterable in the solid state, and forming a solution perfectly clear at first, and remaining so for an indefinite period. The iron has no tendency to a further state of oxygenation; the solution has been agitated with oxygen gas, and kept in contact with it for several days without the least change. A solution of gallic acid tinges a solution of the salt of a light bluish colour, after the lapse of two or three days, and many substances which produce an inky compound with the salts of iron may be mixed with it without causing any change of colour.

The proportion of the three sulphates which has been adopted, is 80 per cent of sulphate of magnesia, 15 per cent of sulphate of iron, and 5 per cent of sulphate of quinine, one scruple containing 16, 3, and 1, grains of the respective salts. These proportions have been found the best for general use, and also for the purposes of manufacture. The proportion of quinine may be increased by prescribing an additional quantity which is readily soluble in the solution of the salt.

One peculiarity is especially deserving of notice: namely, that in this combination, the assisting or adjuvant property of both iron and quinine are remarkably developed, the effect of both, particularly of quinine, being heightened in a very marked manner. At the same time, both of the remedies are less apt to disagree with peculiar constitutions which ordinarily refuse to tolerate either iron or quinine. If the heightened power be borne in mind in prescribing this combination, there will be very few cases found in which it will not be suitable whenever either iron or quinine are indicated.—*Med. Times and Gazette, March* 16, 1861, *p.* 288.

133.—*A New Anæsthetic.* By JOHN WILMSHURST, Esq.—[The article referred to by the writer is the rectified spirit of turpentine ordinarily used.]

The first case in which I tried its effect was that of Mrs. H——,

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1860, *p.* 5

102.—PU

By Dr. J. Y.

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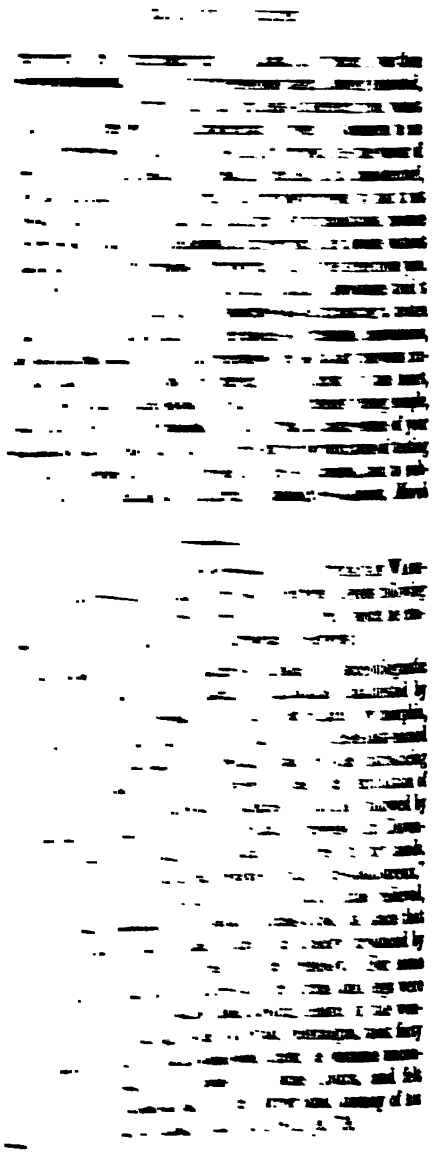


135.—CAUSE OF DEATH FROM CHLOROFORM.

By Dr. JAMES PETRIE, Liverpool.

some cases, at least, the cause of death from chloroform is the tion of the patient during its administration. The usual position ıe prone one, with the face upwards, in consequence of which, ·n the insensibility is complete, the tongue by its own weight falls ĸ, closes the epiglottis, and causes asphyxia. Only in one case the author seen any dangerous symptoms during the exhibition of ɔroform, which he relates as follows :]

The operation being a tedious one, I had left the patient for a ıute or two without chloroform, but still unconscious, and had ıe to see the progress made by the operator, when one of the ıse-surgeons anxiously called my attention to her condition, which s one obviously of imminent danger, but from which she quickly ɔovered by adopting the ready method of Dr. Marshall Hall,—ımely, her tongue, which had sunk deeply into the fauces, was drawn t; she was turned on her side, and her chest was compressed inter-ıttingly, when she began to breathe, and the pulse returned to the ɾist. After a little time the chloroform was resumed in the altered ɔsition without the recurrence of any bad symptom. I have, more pecially since this accident, taken care to give the chloroform in a ɔsition to obviate any casuality from closure of the glottis in this ay.

In a conversation with my friend Professor Simpson, when here bout a couple of months since, on this subject, I expressed my ·pinion to him that the very few deaths which apparently occurred in ıarturient cases from chloroform might be ascribed to the position of he woman. He told me certainly, among some thousands to whom ıe had administered chloroform (and the majority of those must have ɔeen in labour), he had had no fatal case in his practice. Making ıllowance for the tact with which he no doubt gives it, there, ıowever, can hardly be a question that the immunity from accidents which he has experienced must have arisen mainly from the position of the patients.

My object, it will be perceived, is to the bring to the notice of the surgical operator the particular position necessary for avoiding a certain course of danger under the use of anæsthetics; therefore, I would have this matter looked upon as a preventive of untoward effects rather than, as hitherto, one of the means of their removal when they have supervened.

I am aware that in a great many operations on the human body we are obliged to place the patient on the back, but yet, when the lateral position is inconvenient for the performance of an operation, a sitting posture may be adopted, or the head may be turned awry and the tongue thus be prevented from falling back by its own gravity so as impede respiration.—*Med. Times and Gas., Dec.* 22, 1860, *p.* 619.

136.—ON THE TREATMENT OF GOUT.

By Dr. C. B. GARRETT, Hastings.

[Exhausted as we may almost suppose the subject of gout to be, any suggestion for the relief of those terrible twinges in the great toe will not be unacceptable. The following case is extremely interesting.]

Without entering into any discussion on the pathology of gout, it would appear, from the peculiar manner in which tumefaction takes place, that this swelling does not wholly arise from a congested state of the bloodvessels, nor from the œdematous extravasation of fluid in the tissues; for, in the first place, howsoever high the part affected may be elevated, no apparent diminution in size is effected, nor, indeed, any sensible relief produced, nor is there often any pitting on pressure. It would seem that there is to a great extent an emphysematous puffiness, by which the bones and muscles are loosened from their mutual attachments, as is evidenced by the intense pain produced by lifting the foot off a sofa, and allowing it to hang for a moment, and observing the extraordinary expansion which takes place on putting the foot on the floor; and, indeed, this seems to be corroborated by, and to warrant, the following treatment. I shall best elucidate this by giving a case in point.

An elderly gentleman had gout in the left great toe, with considerable swelling of the foot. The pain was excruciating, and the "twinges" most fearful. He screamed out violently if any one approached him, lest even the dress of a lady should come in contact with the toe. His sufferings were more acute than any, I think, I had ever witnessed. I suggested to him that by making steady pressure on the affected part he would gain magical relief. Had I proposed to cut his leg off he could not have been more startled. However, being a man of a resolute mind, he permitted me to place the palm of my hand gently, yet firmly, against the joint, and was not only astonished, but delighted, to find the relief it afforded. He then consented to the use of a bandage, which was thus applied:—Having encased the foot in cotton wadding, so as to make the pressure more easily applied, I passed an elastic bandage twice round the ankle, crossing it, in the usual manner, over the dorsum of the foot, under the instep, and round the heel. I then carried it onwards, and gradually included the ball of the great toe and the toes generally. The pressure being progressive distad towards the seat of disease diminished the nervous sensibility of the part, and lessened the apprehension of the patient.

Immediately after the bandaging my patient fell asleep for the first time for four days and nights, and slept uninterruptedly for eleven hours. The other part of the medical treatment was of the usual character. But there is one other remedy which I rely on more than anything else, and that is, a large mustard plaster over the whole of the abdomen for fifteen or twenty minutes, to be re-applied as often

as the skin will bear it. It instantly gives relief to the gout, it relieves dyspepsia, invigorates ihe digestive powers and the functions of the liver, as well as promotes a healthy action of the bowels, even when ordinary aperients have failed.—*Lancet, April* 20, 1861, *p.* 393.

137.—*Chlorate of Potash and Glycerine as a Topical Disinfectant.*—Experiments instituted at Bicêtre, under the direction of M. Martinet, have demonstrated remarkable disinfecting properties in a mixture of chlorate of potash and glycerine according to the following formula: Chlorate of potash in powder, 2½ drachms; glycerine, 3 ounces; mix. This mixture has been shown, by repeated trials, to present,—1st. A marked disinfecting power, due perhaps to the change which it produces in the secretion, and the mode of action of the wound; 2nd. The property of giving the pus, even when of a serous kind, a greater consistence, often like cream. This result is perhaps, according to M. Martinet, a physical effect of the affinity of glycerine for water, which it subtracts from the pus; but is partly due to the favourable modification which is produced in the suppurating surface. He is inclined to think, that the preparation of glycerine and chlorate of potash may, by thickening the pus, tend to prevent the occurrence of purulent or putrid infection, which generally takes place in connection with suppuration of a serous and unhealthy character. An advantage of the glycerine is, that it prevents the dressings from sticking to the edges of wounds. According to M. Martinet, the glycerine chlorate of potash is not adapted for wounds or sores of a bright red colour, nor for those that are recent or of healthy appearance.—*Edinb. Med. Journal, March* 1861, *p.* 834.

138.—ON THE TREATMENT OF THE DROWNED.

By Dr. CHRISTIAN, Medical Officer to the Royal Humane Society; and Dr. SHARPEY.

[The Royal Humane Society at present use Dr. Silvester's method, whilst the National Life-boat Institution employ Dr. Marshall Hall's "Ready Method." Dr. Christian, in a paper read before the Royal Medical and Chirurgical Society, says:]

After giving the Ready Method a full trial in about 15 cases, the very intelligent superintendent, the boatmen, and the author became so satisfied of its inefficiency to restore animation, and of the difficulty of properly carrying out the manipulations, that he felt himself justified in representing those facts to the committee, and in adopting the plan recommended by Dr. Silvester, which he believed in every way to be superior, more manageable, less likely to injure the patient, will fill the chest with and expel air from it more fully, and will not force the contents of the stomach upwards, and in the way of respiration.

The following are the directions for treating the asphyxiated at the Receiving-house, Hyde-park :—

"Wipe the mouth and nostrils directly the body is taken from the water.

"Use Dr. Silvester's method; at the same time let the body be taken as quickly as possible to the Receiving-house, and place it in the bath up to the neck.

"Raise the body in twenty seconds from the water, and dash cold water against the chest.

"Pass ammonia under the nose.

"Use again Dr. Silvester's method, and the inflating apparatus if it fail.

"Remove the body from the bath, and rub the surface with dry hot towels, perseveringly continuing the other treatment."

After many experiments the author had come to the conclusion that inflation of the lungs by Dr. Silvester's method or by the Society's apparatus is the first remedy, and the shock of the warm bath the second; that after eight minutes' complete submersion recovery is hopeless, and that when ten minutes elapse after being taken from the water without any effort at respiration it is equally so. On the subject of the warm bath, which has excited so much discussion as a remedy, he remarked that it must be understood that it is used as an immediate and powerful excitant; and it had so frequently happened (twice while he was actually present) that a person brought in as asphyxiated, who to the bystanders was apparently quite dead, immediately on being placed in the bath gave the sob or gasp which is the precursor of respiration, that it might be boldly stated to be a most valuable adjunct to treatment, and, properly managed, in no way pernicious. He concluded by citing a letter from Sir Benjamin Brodie to the Royal Humane Society, confirming the preceding conclusions.

Dr. SHARPEY having had the honour of presenting the paper to the society, could not let it pass without remark. His object was, however, rather to elicit opinions on the questions brought forward than to decide on the question, as he had had no practical experience on the subject. Dr. Christian had brought the subject before the society in order that both methods might be well considered, as they were both still *sub judice*. He would observe, with reference to the method of Dr. Marshall Hall, that he had on one occasion spoken favourably of it, but had seen reason to alter his opinion after more mature consideration of the subject, and after hearing the practical experience of the Royal Humane Society, Dr. Sharpey considered that Dr. Marshall Hall's method could only claim one advantage, and it was not clear that it had even that. This supposed advantage is that the tongue falls forwards and thus does not embarrass respiration. He thought that Dr. Silvester's method attained this object without any of the disadvantages of the Ready Method. The disadvantages of the Ready Method were several. It must be remembered that a body submerged

for some time is practically a dead body, and serious mischief has not unfrequently arisen from rough handling. Again, the constant turning of the body renders it very difficult to apply warmth, or carry out the other auxiliary means systematically; but, above all, it does not even fulfil its first object, of changing the air in the chest. Dr. Marshall Hall cited experiments in support of his view; but the want of precision in making them is very striking. He (Dr. Sharpey) could not attach any importance to the results of experiments so conducted. Dr. Silvester has repeated them in a more precise way, and could not get a displacement of more than one cubic inch of air. It would be asked, however, what answer should be made to the many statements of the success of the Ready Method? He could reply, that many of them were in cases of still-born infants, a part of whom, he believed, would recover without any assistance if left to themselves, or at least by very simple means. Then, again, as Dr. Silvester states, there is no air in the lungs of infants. In reference to adults, he (Dr. Sharpey) believed himself right in saying that in many of the cases of recovery after submersion respiration commences spontaneously as soon as the patient reaches the air. If in such cases Dr. Marshall Hall's method is begun at once, it would be unfair to give the credit solely to it. May it not even be, as suggested by Sir Benjamin Brodie, that recovery often follows, not from the means used, but in spite of them? Dr. Sharpey considered that there was just grounds for reviving the old method of insufflation, which was, he thought, given up without sufficient reason. The objection to this plan was that it produced emphysema; but this, as he knew by experiment, was from forcing the air in too quickly, and in too large quantity. Dr. Sharpey then alluded to the various instruments which might be used in inflation. He recommended an elastic bottle of a capacity of about twenty inches. In reply to the objection that insufflation could not always be practised for want of apparatus, he would remark that there could be no reason why the inflation should not be carried on from mouth to mouth. He believed that emphysema, in reference to this class of cases, was a bugbear. Dr. Christian had proved that in unsuccessful cases, in which insufflation had been tried, no emphysema had been found at the autopsy; and a friend had informed him (Dr. Sharpey) that emphysema was never found in the lungs of still-born children who had been unsuccessfully treated by insufflation at the Edinburgh Maternity Charity. The objection that the air would be deficient in oxygen was not practically valid, as the operator might by several deep inspirations quite change the air in his lungs, and then by emptying his chest quickly the air would not have time to undergo much change. There was an outcry against the warm bath, founded on mere speculation. The Royal Humane Society had found it very efficient. The experiments of Edwards and Brown-Sequard were made on puppies and kittens, and were not, he thought,

fairly to be brought into comparison with the use of the warm bath in cases of suspended animation.

[We here reprint, from "*Retrospect*" vol. xxxviii, the Silvester Method of resuscitating persons apparently drowned.]

"The *Silvester Method* is 'a simple imitation of natural deep inspiration, and is effected by means of the same muscles as are employed by nature in that process.' In deep inspiration, we lift the ribs and sternum by the pectoral and other muscles which pass between the chest and the shoulders; so, in the 'new method,' the ribs and sternum are lifted through the intervention of the muscles, *by steadily extending the arms up by the side of the patient's head.* In this way the cavity of the chest is enlarged, a tendency to a vacuum is produced, and a rush of air immediately takes place into the lungs.

Expiration is brought about by simple compression of the sides of the chest by the patient's arms. Thus 'the arms of the patient are to be used by the operator as handles to open and close the chest.'

The following rules are to be observed in inducing artificial respiration by this new method.

1. *Position.*—Place the patient on his back, with the shoulders raised and supported on a folded article of dress.

2. *To maintain a free entrance of air into the windpipe.*—Draw forward the tongue, and keep it projecting beyond the lips. By raising the lower jaw the teeth may be made to hold it in the proper position.

3. *To imitate the movements of respiration.*—Raise the patient's arms upwards by the sides of his head, and then extend them gently and steadily upwards and forwards for a few moments. (This action, by enlarging the capacity of the chest, induces inspiration)

Next turn down the arms, and press them gently and firmly for a few moments, against the sides of the chest. (Forced expiration is thus effected.)

Repeat these measures alternately, deliberately, and perseveringly, fifteen times in a minute." (p. 404.)

[Dr. Marshall Hall's Ready Method, as published by the National Life-boat Institution will be found in "*Retrospect*," vol. xxxviii, p. 403.] —*Lancet, Feb.* 2, 1861, *p.* 111.

139.—THE TURKISH BATH.

By Dr. R. H. Goolden, Physician to St. Thomas' Hospital.

[The following are extracts from a most able letter written to the authorities of St Thomas' Hospital, by Dr. Goolden, in answer to a circular addressed to the Medical Staff, requesting their opinion upon the Turkish Bath. Speaking of the effects observed by him in a bath much resorted to by the lower orders, the writer observes:]

The most marked relief was found in cases of gout, rheumatism, periosteal nodes, and sciatica. Some cases of Bright's disease were more relieved by the sweating than I had ever seen; but they produced a most distressing effluvium in the bath, so that it was necessary to change the entire atmosphere of the hot room. Eczema was soon relieved; psoriasis much benefited, but required a longer and more frequent bath. Bronchitis was relieved at once in many cases; but it required several baths to cure those cases which were of any long standing. Pleurisy, the only case I saw, received no benefit, and suffered very much distress. Several cases of phthisis said they felt relief; the cough was quieted, and the night sweats relieved, but I saw no permanent benefit. If any injury supervened, I was not likely to be made acquainted with it, as the patients would not return to the bath; but I should fear that where cavities are formed, hemorrhage or bleeding from the lungs would be very likely to result. My inference is drawn from a single case, to which I shall allude presently. I saw a number of children suffering from scarlet fever who were brought down daily. I will not pledge myself that the bath exerted any beneficial influence; but the cases, many of which were very severe, *all* recovered rapidly, and left no serious results. One case of typhoid fever was much relieved. This is what one might expect, because this form of fever is unknown in the tropics; but what struck me as most remarkable was the influence of the hot room in quieting the circulation in some cases of palpitation of the heart. Some cases that I saw were decidedly injured by the bath; but they were such as one would expect would be so injured. One was a case of diseased liver, who suffered pain in the right shoulder, and supposed that pain to be rheumatic; but, on the other hand, I have seen cases of severe dyspnœa and sluggish liver most satisfactorily relieved by it.

I have been speaking of cases concerning which I had no responsibility. I was allowed to watch, but had no right to interfere.

In my private practice I have prescribed these baths, and I have found them to afford very great relief in some cases of chlorosis (green sickness), and in a form of chronic inflammation of the stomach common to young women. Common catarrh is cured at once; and the weekly use of the bath diminishes, or altogether suspends, liability to the attacks. Common catarrh includes what are called colds in the head, quinsy, sore-throats before suppuration, the common winter coughs, and some forms of diarrhœa. Pains in the muscles after excessive and unusual fatigue are removed; and those who suffer pains in the seats of old injuries upon slight changes of the weather, lose those pains entirely.

The influence on gout is very remarkable. During the acute attack the pain entirely leaves the patient when he has been some time in the hot chamber, and during the perspiration; but the pain usually returns as he cools down afterwards, though with considerably less intensity, and each bath perceptibly improves the patient. I find,

however, that draughts of a solution of bicarbonate of potassa and nitre greatly facilitate the effect of the bath.

A near relation of mine, who had led a hard life of exposure to various climates and great fatigue in the Queen's service, and had retired on account of ill-health, suffered attacks of angina pectoris, became greatly emaciated, dropsical, and was seized in addition with an attack of bronchitis. His appetite entirely failed him; and Dr. Watson and I considered that he was rapidly sinking. He had had no regular sleep for several weeks, waking up in a state of alarm as soon as he dropped off to sleep; and he was then unable to walk across his room. In this state, with Dr. Watson's approval, I took him to the bath. (He had had baths both in Turkey and Russia). I had great difficulty in getting him in and out of the carriage, and in supporting him down the stairs. When he was in the hot chamber, he soon broke out into a profuse sweat, and said he had not felt so comfortable for a long time. He remained there above an hour, and I had great difficulty in persuading him to leave it, he felt so much relief. The dyspnœa (difficult breathing) was gone; the palpitation of the heart had subsided to a natural action; he rode home without inconvenience from Bell-street, Edgeware-road, to Chester-square, Pimlico; he enjoyed his food, and slept incessantly for nearly three days and nights. From that time he rallied; and although still subject to attacks of angina, is in the enjoyment of comparatively good health, and capable of attending to his private business.

A clergyman (a country rector) complained to me of rheumatic pains, and susceptibility to take cold on any slight exposure, which interfered with his clerical duties. I recommended the bath to be used twice a week. Evans persuaded him to use it every day during his stay in London, and he persevered in its daily use, although all rheumatism was gone. It so happened that he had been a midshipman in early life, and had received a stab with a knife from a Chinaman in some former contest at Hong Kong. The blade wounded the lung, but the wound was healed, and he never afterwards suffered the slightest inconvenience from it. However, whilst he was taking these baths, he was seized suddenly with spitting of blood. The old wound in the lung re-opened, and he nearly died from the hemorrhage; but he recovered.

This is the only case in which I have seen any serious consequences follow the use of the bath. It is important, however, to record it, because it would indicate some danger in cases of advanced phthisis, where cavities have been formed. I have several times seen boils follow the employment of the bath, but they never caused more than a temporary inconvenience.

I have given a fair and not over-stated account of the results of the bath used as a remedy for disease—sadly defective, I confess; but I hope to complete it by observations made in this hospital when the

governors will afford the opportunity. There are, however, some points to be touched upon with respect to its construction and use.

The hospital bath should consist of several chambers: a hot room (150°), a tepid room (100°), and an ante-room, with a supply of hot and cold water, so arranged that the bather can have a shower bath or douche at any temperature. There should also be a tap of cold water supplied to each room. The rooms should be warmed by a flue, not necessarily under the floor. In attending to this matter, much saving may be effected without lessening the efficiency. The floor should be of brick, and the walls may be of cement, brick, or Dutch tiles; and the rooms must be so constructed as to admit of perfect ventilation. The estimated cost of fuel is about 1s. 6d. a day.

It requires some care in giving the bath for the first time, because the skin does not always readily respond to the heat by perspiration, and the patient experiences a sensation of faintness. The faintness is, however, more apparent than real, for the pulse keeps up, and the distress subsides as the skin begins to act. Whenever this distress is felt, the patient should have some cold or warm water thrown over the skin, or even be allowed to go for a minute into the cold air. They *never* take cold.

The atmosphere in the hot room should *not* be perfectly dry, otherwise it causes much distress to some persons, and the perspiration is retarded; for the skin must be moist to perspire. The air cells of the lungs also require moisture for the respiratory action; otherwise the patient would be suffocated, even in pure oxygen, as the fish dies in the atmosphere. Too rapid an evaporation therefore is to be avoided, and likewise an atmosphere saturated with vapour at a high temperature is unfitted for respiration, because the vapour displaces too large an amount of air. Mahomet has introduced into his bath a small jet of steam, which is easily regulated, and determines the dew point at its proper degree.

If you hear some of the advocates of the bath talk about its action on the human organism, you will probably hear much questionable physiology. They tell you that the skin does the action of the lungs, and breathes. I believe this to be a fallacy; but of this I am certain, that the skin was designed by nature to be a great organ for excretion. In the purest perspiration there is a large amount of urea, the same material so largely eliminated by the kidneys. This is a fact I know from chemical analysis, and it explains the great relief produced by the bath (i.e., the air bath) in cases of Bright's disease, where the urea retained in the blood is producing poisonous effects; it also accounts for the strong and offensive smell. This same urea, resolved into carbonate of ammonia, produces the distressing smell of a crowded room, and the carbonic acid of that carbonate of ammonia has misled some of our physiologists to believe that it is the result of cuticular respiration.—*Lancet, Jan.* 26, 1861, *p.* 95.

140.—THE HOT-AIR BATH.

By Dr. B. W. Richardson, M.A.

From the use of the heated air bath as a therapeutical agent to its use as a social enjoyment or luxury, is a wide step, and a step, which I for one, hold back from taking. It seems to be the misfortune of this remedy, that its administration is for a time attended by a sensation of great pleasure and satisfaction. This is not a peculiar feature of the heated air bath. In the palmy days of blood-letting, when every one was periodically bled, the argument in favour of the practice was, that it produced for a time a lightness and an agreeable feeling. I absolutely knew a man, who for years had been cupped once a month regularly, for no other reason than that the operation was followed by sensations of lightness and ease. Persons who are recovering from a long and depressing illness, or who take purgatives, experience the same agreeable feeling. In the respiration of nitrous oxide gas, and even in a certain stage of opium-narcotism, the same mental liberty is experienced. But what is the meaning of this sensation? Is it one of power? I believe not. I believe it is simply a transitional decline, either from a state of artificial exaltation to the natural life, or of decline from the natural life to a condition below it—conditions of unconsciousness and temporary or absolute oblivion; even in the passage to death itself, there is a transitional stage of pleasure, if physical local pain be not present to interfere. I take it, then, that this luxurious state, as extorted by the heated air bath, is one of depression; a state, perchance, valuable in disease, and in health harmless as a mere experiment, but ruinous when driven into an intemperate and constant habit.

I deny that this bath can ever take the place of exercise, or in any way supplant exercise. I urge that in all cases the time required for the bath in healthy men were better spent in digging, walking, rowing, the cricket field, the skittle-ground, or the rifle drill; and I regret to see even an attempt to introduce this luxury as a substitute for these occupations. I look back into history, and I am more fully preserved in this belief. *Turkish* and *Roman* are words of evil omen. I recall the histories of Turkey and Rome. I recall that in the periods of the rise of these nations a ruler lived in a cottage and tilled the ground, or a caliph clothed himself in an undressed skin, and washed not once a week. I recall that when these nations reached their glory they were more civilised, but yet not luxurious; and, lastly, I recall that when these great nations collapsed, and were beaten by inferior tribes, luxurious tastes had crept in and destroyed them, whereof this bath was the most prominent and enticing of all.

I predict that it will be the same here under the same principles. I predict that, whenever Englishmen give up the active occupations I have named, and in slippered pantaloon luxuriate daily in a bath to rid themselves of the products of exertion, then this country will have

passed the zenith; then there will be no great hero to bid every man do his duty, no man to do the duty, and no England for which the duty should be done.—*British Medical Journal, Feb.* 2, 1861, *p.* 114.

141.—THE LAMP BATH.

By Dr. CHARLES TAYLOR, Nottingham.

Apropos to the discussion which has recently occupied a place in your columns on the Turkish baths, permit me to call the attention of your readers to a very simple method of exciting the emunctory function of the skin, in my opinion quite as effectual, and possessing advantages peculiarly its own. I allude to the lamp-bath, which is thus applied. The patient, *in puris naturalibus*, is seated on a common wooden chair with his feet upon a low stool, the body is then enveloped in two or three blankets, the head being excluded, and a large spirit-lamp placed under the seat; in a few minutes the skin becomes moist, and in about a quarter of an hour the perspiration streams down the body and is collected in beads upon the face; this is kept up so long as may be considered necessary, the patient meantime drinking one or two tumblers of water, when the blankets are rapidly removed, and he is subjected to a douche of two pails of cold water, and dried with much friction, after which a smart walk in the open air may be taken. The process does not occupy more than three-quarters of an hour, and may be applied at any time in the invalid's bedroom. I believe the profuse perspiration thus excited, and which may be carried to any reasonable extent, effects all the good that can be accomplished by the *calidaria* of the Turkish baths, while its extreme simplicity and portability ensure for it advantages as a therapeutic agent that those institutions can never command; among the more important of which, I would enumerate the fact that during the whole process the patient breathes cool air instead of that of an apartment heated to an extent necessary to produce such an effect, and indeed may, as I have seen in the summer time, be placidly seated regarding a beautiful landscape from an open window during its administration; thus the ill effects of respiring hot air experienced by many even in over-heated rooms, such as difficulty of breathing, violent palpitation, giddiness, faintness, &c., are all avoided; while the fact that it is used in the patient's own apartment, and may at first be moderated to any extent that may suit his fancy, materially lessens the risk of disagreeable nervous symptoms very likely to be experienced in invalids; who, moreover, are frequently unable or unfit to visit the public baths, or unwilling to expose their infirmities to the gaze of strangers, who, I may observe, *en passant*, would be little likely to patronise establishments where they were liable to meet as "com-

panions of the bath" persons suffering from lepra, psoriasis, dropsies, influenza, &c.

I have seen the above process attended with the very best effects in some cases of obstinate skin-disease, in rheumatism, œdematous swellings, catarrh, syphilis, albuminuria, torpor of the liver, &c.; and have more particularly noted its good effects in cases where individuals, from leading a sedentary and town life, had grown fat, pallid, flabby, and altogether out of condition. Such speedily loose fat, and gain muscle: the complexion clears; the colour heightens; the skin becomes smooth and polished. They experience a feeling of lightness and buoyancy to which they have long been strangers, and soon add to the appetite of a wolf the digestion of an ostrich. It is seldom omitted in the treatment of persons subjected to what is known as the water-cure, and has been employed for years in the hydropathic establishments to which used-up men about town notoriously resort, and with the best result, to recruit their injured health and shattered frames. By fixing a small sauce-pan, containing water, over the lamp, it is converted into a vapour-bath, medicated or not; and may be used for sulphureous and mercurial fumigations.

I must confess that I cannot see the force of the objections that I have heard urged against the employment of this simple remedy. We are told—1. That the patient is surrounded with an atmosphere deprived of its oxygen and loaded with carbonic acid, the product of the combustion of the spirit of the lamp; and that this, by interfering with the respiratory function of the skin, must have a most deleterious effect; 2. That the atmosphere surrounding the patient is stagnant, and does not circulate; and 3. That it is an advantage for the patient to breathe the heated air, as was done in the baths of the ancient Romans.

With regard to the first, it appears to me to be of very slight importance if the air were loaded with carbonic acid, as the respiratory function of the skin, while bathed in perspiration, which is coursing on all sides in little rivulets, in quantity sufficient to saturate the blanket or cushion on which the patient is seated, and drops from the footstool to the floor, must be reduced to nothing; but I cannot admit that the patient is surrounded by such an atmosphere, as the specific gravity of the carbonic acid would ensure its falling below the level of the chair on which the patient is seated. In answer to the second, I contend that the air surrounding the patient is not stagnant, but circulates freely; cold air, according to a natural law, rapidly taking the place of the hot as fast as heated, which must necessarily be the case, unless the patient were hermetically sealed up, when the combustion of the lamp could not be supported. With regard to the last, I own that I am sceptical as to the benefit to be derived by a delicate invalid from inspiring air hot enough to cook a chop.—*British Medical Journal, Nov.* 24, 1860, *p.* 927.

142.—ON A CASE OF PROPTOSIS, GOITRE, PALPITATION, &c.

By Dr. C. HANDFIELD JONES and Dr. C. J. B. WILLIAMS.

[The following case was recently brought before the Royal Medical and Chirurgical Society in a paper read by Dr. Handfield Jones.]

C. B., a woman aged forty, had been on two occasions under his (Dr. Jones's) care, suffering from excessive prostration induced by over-work, proptosis, vomiting, cough, more or less palpitation, and thyroid enlargement. On the first occasion the throbbing and enlargement of the thyroid were so great that the question of ligaturing the arteries was entertained. The debility was so marked that her life was in considerable peril. Iodine internally and externally had no effect in diminishing the goître; leeches gave very temporary relief; but the application of ice was very beneficial, reducing the volume of the gland and checking the throbbing of the arteries. In the first attack nitrate of silver was eminently serviceable in allaying the irritability of the gastric mucous membrane, and then tonics were administered with great advantage, so that she was comparatively restored to health. In the second attack, which occurred at the interval of about a year, the free administration of opium at first, and afterwards of strychnine, quickly restored her. Country air had always been of very marked benefit. The proptosis disappeared or greatly lessened as she improved in health. The view taken by the author was that the fundamental malady was debility, especially of the nervous system, which, by affecting various vaso-motor nerves, gave rise to the several symptoms. Thus effusion behind the globe would cause proptosis; hyperæmia and increased action of the cell elements of the thyroid would produce goître; and paresis of the vagi would give rise to palpitation and vomiting. The benefit derived from tonic remedies strongly corroborated this view.

Dr. C. J. B. WILLIAMS said he knew of no class of cases more curious, or more peculiar in their prominent symptoms, than that referred to in the paper. There was none in which the effects of treatment were more striking. At first, these cases seemed to present all the signs of excessive arterial action, requiring depletion, sedatives, or other lowering treatment; but we now knew that they were only to be combated by powerful tonics and general support to the system. Their pathology had at one time puzzled him (Dr. Williams) much. Many cases had been presented to him under the garb of heart disease or consumption. The physiognomical character of the disease was too remarkable to be mistaken. The prominence of the eye was striking: the eyeballs projected beyond the brows, sometimes to such an extent that the eyelids did not close during sleep. The vessels on the forehead, face, head, and neck, were more prominent, throbbing, and larger than natural. This feature led him to regard the pathological character of the disease as consisting mainly in a kind of aneurismal varix of the

thyroid. Enlargement of vessels would account for the prominence of the eyes, which were also, probably, in some cases rendered more prominent by effusion into the orbit. The whole cerebral circulation was in the same state of enlargement, producing excitement and other symptoms of disturbance of the brain. This state depending on diminution of nervous power, and not on increased power, explained the use of tonics in the disease. The action of tonics was remarkable. It might be necessary in some cases in the first instance to allay pain and excitement by the use of sedatives; but it was surprising how well the patient bore tonics when so much excitement prevailed. He had found the best tonics to be those of a more astringent kind, such as the perchloride and the phosphate of iron, which acted almost as specifics in the disease. Formerly he had been in the habit of using the nitrate of silver, and other of the milder tonics; but little benefit ensued until the more decided course of action was persevered in. This, with the occasional use of narcotics at night, highly nutritious food, pure air, and a careful husbanding of the strength, soon effected a beneficial change. He related the case of a lady to whom he was called in the early stage of this affection (he might here remark that all the cases which he had seen occurred in females). This patient was highly nervous, and suffered from great excitement and pain of the head; the eyeballs seemed starting from their sockets. Temporary relief was obtained by the exertion of pressure on the eye, and binding a handkerchief tightly round the head. There was enlargement of the arteries of the forehead, face, and neck. Was this a case for tonics? The answer was in the affirmative. Sulphate of iron with an excess of sulphuric acid was ordered. Under this, with a local application of Fleming's tincture of aconite, the patient perfectly recovered in a few days. He inquired if any member had examined, post mortem, many of these cases; and suggested that the arteries of the brain and the orbit should, when possible, be carefully scanned.—*Lancet, Dec.* 8, 1860, *p.* 562.

143.—*Simple Mode of Sedative Hypodermic Inoculation.*—M. LAFARGUE has published in the Bulletin de Thérapeutique (Jan. 15 and Feb. 28, 1861) two articles on these inoculations, and advises the use of a little instrument composed of a hollow needle and plug. By means of the latter, a moistened powder, composed either of atropine or morphine and powdered belladonna, is forced into the areolar tissue, the latter taking up the substances very rapidly. In the absence of the regular hypodermic syringe, or the instrument just mentioned, M. Lafargue thinks that neuralgia may be relieved by using an ordinary lancet, as is done in vaccination. The powder should be placed in a watch-glass, and moistened with a drop of water. In this kind of paste the lancet is to be dipped, and from two or three to a dozen punctures be made over the course of the nerve. The operator should

each time be careful to turn the lancet in the little wound made by the instrument. If this mode is found to answer, it will soon recommend itself by its very great simplicity.—*Lancet, March* 30, 1861, *p.* 330.

144.—ON TRACHEOTOMY.

By C. F. MAUNDER, Esq., Assistant-Surgeon, London Hospital.

In the performance of tracheotomy, the introduction of a canula under circumstances requiring its use is usually deemed to be the most difficult step in the operation—a difficulty due to the constant and rapid elevation and depression of the trachea during respiration and deglutition. The difficulty is especially felt in children, in whom the windpipe is small and yielding. To overcome this obstacle I would beg to suggest a procedure which will facilitate the completion of the operation.

The trachea having been incised longitudinally, the operator should insert the point of a *double* hook through the incision into the windpipe, and hold the latter elevated and fixed; he should then slip up the clasp and allow the halves of the hook to separate by their own elasticity, and so to widen the slit in the trachea; this done, the canula may be passed into the tube with comparative ease, and the hook withdrawn.

Should any difficulty be experienced in the attempt to open the trachea after division of the softer tissues, the organ may be fixed in the adult by holding the cricoid cartilage firmly with the finger and thumb, or by a sharp hook inserted into the latter cartilage through the upper angle of the wound. In the child the sharp hook should be used.

Although a double canula be used and the inner one be removed at intervals, cleaned, and replaced, still mucus becomes inspissated, and, adhering to the extremity of the larger tube, offers a serious obstacle to respiration. This inconvenience may be remedied, while the inner tube is being cleaned, by the careful introduction of a small elastic catheter—well warmed, so as to soften it, and oiled—quite through the larger tube just into the trachea. By this means a passage is cleared and the obstruction is removed.

Again, although a double canula be used, mucus collects at the further extremity, and cannot always be dislodged either by cleaning the inner tube or by a catheter or feather; both must therefore be removed and cleaned, or be replaced by others. This necessity occurred to me in a case recently under my care; and in order to obviate the difficulty anticipated on re-introduction of the cleaned tubes, I first removed the inner canula, and then passed a small elastic catheter, well softened in hot water, through the outer tube into the trachea, and, maintaining it there, withdrew the tube over the catheter, and, as soon as it was cleaned, passed it back again along the catheter, still in the trachea, till the former occupied its orginal position; the catheter was then removed from the trachea.

The catheter served both as a tube by which respiration could be sustained, and also as a guide for the re-introduction of the cannula, after the manner of Wakley's catheters.—*Lancet, March* 16, 1861, *p.* 262.

145.—A PECULIAR SYMPTOM PRODUCED BY THE USE OF TOBACCO.

By Dr. JONATHAN OSBORNE, Physician to Mercer's Hospital, Dublin.

[The following passage we extract from an able paper by Dr. Osborne, on the "Involuntary Actions of Voluntary Muscles." Speaking on the subject of the effects of tobacco smoking, he observes that there is one peculiar symptom which he has never seen described elsewhere.]

It is this: the smoker feels a pain, dull, and, although not acute, yet distressing, from the constant sense of oppression which attends it. Its seat is behind the breast bone, and rather tending to the left side. It is neither aggravated nor diminished by full inspiration, nor by any change of posture; comes on usually in the afternoon, but irrespectively of meals, and sometimes increases in severity during the night, so as to interfere with sleep, yet almost completely vanishes before morning, and this without any appreciable disturbance of the circulation, respiration, or digestion. My attention was first directed to it by a gentleman, at that time attending the clinic at Sir Patrick Dun's Hospital, who having contracted the habit of smoking, suffered much from this pain, which resisted every remedy till he gave up his pipe altogether. Within less than a month afterwards it ceased, and never returned. I have subsequently met several similar cases, and in some of these ordered only the most insignificant medicines, but always insisted on a total abstinence from smoking, and in every instance, as far as I have been able to ascertain, with uniform success. This affection, in all my cases, occurred before the middle age of life, did not appear to be produced by any one kind of tobacco more than another, attacking indifferently the votaries of Cavendish and Mundungus. From the vagueness of the seat of pain and its transitory character, it would appear to be a semi-paralytic, and consequently a neuralgic, affection of the central and anterior portions of the diaphragm.—*Dublin Quarterly Journal, Nov.* 1860, *p.* 291.

146.—ON CYSTIC DISEASE.

By Dr. MAURICE H. COLLIS, Surgeon to the Meath Hospital.

Of growths which are not cancer, but which equally with it have their origin in the lymph-cell, and are interstitial or infiltrating, there are almost as many varieties as there are cases. After reading most

of what has been written about them, and having seen a good many, I am satisfied that this whole class may, for practical purposes, be included in the following formula:—"*The nearer in form and power of development that the constituent cells of a tumour are to the healthy lymph-cell, the more innocent is the tumour: the further removed, the more destructive.*" To expand this a little, and make it intelligible, we find the healthy lymph-cell small, circular, slightly granular, with a little nucleus, and developing into a fibre. Our simplest tumours are composed of cells, scarcely, if at all, to be distinguished from the above; and these white fibrous, or desmoid tumours, are the most innocent possible growths, as a general rule. A stray exception may occur, now and then, to prove the rule. We then come to fibroid, fibro-nucleated, recurrent fibroid, fibro-plastic, fibrinous tumours, named according to the fancy of writers, who recognize alike their similarity to simple fibrous tumours, and their divergence from them. These are of variable malignancy; they are of as variable minute construction. Not only do their constituent cell-elements differ more or less in form from the primary lymph-cell, but they also differ in power of development. Some remain always as cells, and never develop into fibres; these are the most recurrent. Some make attempts at development, and hence the caudate cell of various form; some appear as nuclei, only without external cell-wall. Again, power of development into fibrous forms is quite different from active reproduction; generally it is not associated in the same cells. The recurrent tumours are masses of rapidly produced cells, or nuclei, with no attempts at the formation of fibre.

Of these I have seen many examples, none more remarkable than the case of a man from whose shoulder such tumours have been removed, at least twenty times, by Professors Rawdon Macnamara, senior and junior, and by other surgeons. My friend, the present Professor of Materia Medica to the College of Surgeons, knowing the interest I took in such subjects, kindly gave me an opportunity of seeing the tumour on one occasion. It was a mass of most elementary cells, low in organization, active in reproduction, and was the most recurrent tumour I ever heard of. There is some tendency to this form of growth in all secondary tumours, whether glandular or local relapses.

Cancer itself can be brought under the above law. It is no heterologous or parasitic formation. It is simply a monstrously abnormal plastic growth; its cells differ as widely as possible from the healthy type. In acute cases they are rapidly produced, make scarcely an attempt at development, and die off with rapidity; in schirrus they are formed more slowly, and in much smaller numbers, live longer, and make some attempt at caudation, but they are still farther removed in form from the typical cell of healthy tissues.

The more I think over the subject of morbid products, the more am I convinced that, in the above formula, we have the expression of a law that includes most of their phenomena. It will, slightly modified, apply not only to large classes of tumours, but also to tubercle and to pus. Tubercle is a lymph-cell, of low vitality, incapable of development into healthy fibre, dying after a short existence, and generally becoming a foreign body. Pus may be described in words almost identical—their material difference being one, probably, more of chemical constitution than of vital power—for both are possessed of almost a minimum of vitality. Tumours are composed of cells whose vital force is greater than pus or tubercle; and this vital power is rather spent in reproduction, than in development, as in the healthy cell. The ordinary plastic cell goes through certain phases, dies, and is removed; its place is taken by a new cell, developed, probably, from the nucleus of its predecessor. The abnormal cell fails to arrive at perfection, often becomes a monstrosity, and has a tendency not only to reproduce itself from its nucleus, but to generate in neighbouring lymph organisms similar to itself. Thus the constituent cell of the tumour has a certain independent vitality, similar to that of the entire tumour; or rather the converse is true—the tumour is composed of cells of independent vitality, and hence it possesses the same form of life with the cell. And, as the life of the cell is of a low type, so is that of the tumour. I cannot but think that many of the able minds which have been engaged in studying cancer, in its minute anatomy, have failed to make an adequate impression upon practical surgery, because they have been led away to look on cancer as a thing quite different from any of the ordinary structures of the body, instead of a perverted form of a natural structure. And I am sure that their views, carried out to their legitimate conclusion, would lead us to despair of any remedy for cancer but the knife, or other agents of destruction. Whereas, if we regard the cancer-cell merely as a perverted lymph-cell, we shall never rest until remedies are found which will influence it to a more healthy type.

I am sure we neglect too much the auxiliary treatment of good tonics, especially mineral tonics, good air and abundant food, with proportionate exercise; and we should have slower tumours, and fewer relapses, if we compelled the attention of our patients to these matters.

Whether chemistry, combined with the microscope, will ever lead us to the knowledge of how best to effect our purpose, is more than I can say. The study will, at all events, lay open many valuable truths—it will amply repay any one who can give time to pursue it. The actions of many medicines, may I not say most, are involved in obscurity; and we prescribe them on grounds which our utmost efforts cannot rescue from the taint of empiricism. Microscopic chemistry will solve some, at least, of these problems. For example:—The

diuretic power of bitartrate of potash is explained when we see the force with which it dissolves the epithelial cells of the kidney; its value in certain conditions of the liver is similarly explained. Ether is not only useful for its stimulant properties (the result of its rapid combustion, as chemistry has taught us), but also for its expectorant action, which is due to its solvent power over the bronchial epithelium: for this knowledge we are indebted to the microscope; and many other valuable results will reward those who apply themselves to it.

A word or two with regard to local treatment, after operation for the removal of tumours. A rapid cure is of the highest importance: it is obtained with less inflammatory action, and is so far less favourable to the plastic exudations in which secondary growths will spring up. I do not believe that suppuration is at all beneficial as a preventive of relapses, nor can I conceive on what imaginable grounds such a theory could rest. To obtain union as rapidly as possible, it is not enough to secure all bleeding vessels at the time; we must also prevent the possibility of blood lodging under the flaps; and we must do this without exerting such pressure on the latter as will either provoke reaction or interfere with healthy circulation in them.

The mode of dressing I adopt is as follows:—Let us suppose an amputation of the breast. I insert a wire suture at every inch; having twisted them, I cut the wires off at about an inch from the twisted portion; when this is completed, and the edges of the wound lie nicely in contact, I lay along each side of it, and parallel with it, two large compresses of lint filled with soft cotton, taking care to get their edges under the ends of the wire: these compresses are for the purpose of making elastic pressure all along the sides of the wound, thus preventing any accumulation of blood under the flaps; and they also interpose between the skin and the cut ends of the wire sutures, and prevent chafing; over the track of the wound, a wet compress can be laid, and the whole secured by a few turns of a roller, applied with moderate tightness. If there has been any dissection of glands from the pouch of the axilla, a sponge rolled in lint, or a handful of cotton, can be similarly used, to prevent accumulation of blood; and when the arm is brought to the side, sufficient pressure is made. A little ingenuity will adapt this mode of dressing to many operations. I have used it in amputations of limbs, castration, the removal of all tumours, and for the dressing of scalp wounds (omitting the stitches, or inserting as few as possible), and I have found it most useful. One advantage it possesses is, that we can easily inspect the line of wound, and sponge it clean without disturbing it; blood in the wound is the great cause of delay and danger after all operations—it prevents adhesion, causes suppuration, and tends to produce phlebitis and purulent absorption, or angeioleucitis; and, as this mode of dressing prevents its accumulation, it so far diminishes the risk of operating.—*Dublin Quarterly Journal, Nov.* 1860, *p.* 369.

147.—ON THE MODE OF ACTION OF ALCOHOL IN THE TREATMENT OF DISEASE.

By Dr. EDWARD SMITH, L.L.B., F.R.S., Assistant-Physician to to the Hospital for Consumption, Brompton.

[The following is a summary of the late Dr. Todd's views on the subject of the therapeutic action of alcohol. His views are based upon the chemical theory that alcohol is transformed within the system and produces heat.]

1. Alcohol acts primarily on the nervous system, and, like other hydro-carbons, but in a greater degree, has great affinity for the nervous system.

2. It acts in two degrees; one beneficially, when it augments the generation of nervous power; and the other injuriously, by deteriorating, impairing, or destroying the nutrition of nerve-matter. In the first degree, and when it acts beneficially, there is no smell of alcohol in the breath; but in the latter, or when the dose is too large, the odour is perceptible.

3. There is no true secondary depression of the vital powers, except when the quantity is too large, and then it acts by deranging the digestive functions.

4. Alcohol does not, in any dose, cause inflammation of the lungs, heart, or liver. The brain symptoms do not show congestion or inflammation, but a poisoning of the nerve-cell and nerve-fibre.

5. When carefully taken, it upholds the calorifying process, strengthens the action of the heart, and reduces the frequency of the pulse. By upholding the calorifacient process, it prevents oxydation of the nervous and other tissues.

6. It is simply absorbed into the circulatory system; but oil (another hydro-carbon) has a more complicated digestion, and therefore alcohol acts more quickly and certainly.

7. The dose should be from two drachms to two ounces, and repeated as frequently as food would be given, and with the same intention—viz., to save the tissues from oxydation. The dose and the frequency should be precisely adhered to.

8. The general use of alcohols shows an instinct in man for them, and in recovery from disease they uphold nervous force, and supply the most assimilable material for combustion. They calm the nervous system, and avert delirium.

9. It is very dangerous to withdraw the alcohol, but the signs which would sanction this are referable to deranged digestion—as flatus, eructations, sickness, and dry tongue and mouth; and also the smell in the breath, which implies that the alcohol is passing off unchanged. Coma and delirium from excess of alcohol rather indicate that more alcohol should be given.

[Dr. Smith proceeds to remark:]

Such is a summary of Dr. Todd's views, and they chiefly include the belief that alcohol increases nerve-nutrition, strengthens the heart's action, lessens the rapidity of the circulation, shields the tissues from oxydation, and produces heat; and the sign which measures the efficiency of it is the non-existence of the odour in the breath. As to the skin, he merely remarks that when alcohol is given to a patient who has a hot skin and rapid pulse, it may look like an illustration of the dogma, "Similia similibus curantur." In reference to all his inferences from the supposed transformation of alcohol in the system, we may remark that they are of no value if it be shown that this transformation does not occur, but that alcohol, after remaining in the system for a time, and disturbing its action, is ejected still as alcohol. I believe that the latter is almost, if not quite certain.

I see no ground whatever for the statement that hydrocarbons have an especial affinity for the nervous system, taking starch and oils as the representatives of this class, and Dr. Todd does not adduce any. Neither has it been in any way proved that alcohols improve the nutrition of the nerve-cell and fibre in any separate and special way, but only by improving the whole nutrition of the body. Indeed, the phenomena of nutrition lead our attention away from mere local actions as independent conditions, and teach us that it is a general and not a local act. All the phenomena attending upon the action of alcohols, when taken in quantities which may render their effects noticeable, evince great nervous disturbance: and I cannot but believe that, whilst the nervous system is principally affected, it is by the opposite of a healthy and nutrient action.

The statement as to the action of the heart is doubtless correct, as is also that upon the frequency of the pulse; but the latter action is indirect, and probably due to the former, whereby a larger quantity of blood is forwarded at each contraction of the heart, and the blood is more perfectly distributed to the whole body. A rapid pulse is commonly a small one, and the impulsion of a large quantity of blood at a time lessens the necessity for the quicker circulation of it.

It is singular that so acute an observer should have made so little reference to the effect of alcohol upon the skin, its power to accumulate water in the body, and to retard the elimination of urea; but in reference to the former I may remark that acetate of ammonia was a most frequent adjunct to his alcoholic treatment, and this might do much to counteract the action of alcohol upon the skin. Moreover, whilst he did not withhold alcohol when the skin was hot and dry, his cases were frequently such as had perspiring skins, and in such instances he remarked upon the diminution in the perspiration during the action of alcohols. It may also be here observed that Dr. Todd most commonly added chloric ether, and not unfrequently quinine, to his plan of treatment, and hence there were several actions induced, some co-ordinate and others opposed.

His statement in reference to the absence of alcoholic exhalation

from the lungs, in cases of disease in which the alcohol is well borne, is worthy of further investigation; but on inquiry I do not find that this fact has been recognised by others. If alcohol pass off untransformed, it is evident that this cannot be true; but if it be true, it is a most valuable guide.

Upon the whole, I do not think that the arguments used by Dr. Todd are now sufficient to establish his theory of the action of alcohols, or to warrant his peculiar plan of administering them; but, on the other hand, I think that the practice which he pursued must rest only upon the ground of his personal authority.

It now remains to state the conditions and stages of disease to which alcohols in various forms, seem to be well fitted,—the action in health being accepted as a sufficient guide,—and it will not be difficult to show that the usual practice of the profession has been based upon truth, however little or much the mode of action may have been understood.

I think that it must be accepted that the *essential and direct actions of alcohols, as remedial agents, are, the increase of the force of the heart's action, the local stimulation of the stomach, and the diminution in the action of the skin;* whilst the most dependent actions are, the retention of urea and fæces, by, or with, retention of fluid, diminution of certain secretions, and derangement of the assimilative process.

In conditions in which the force of the circulation is too feeble, there must be defective nutrition, innervation, and oxydization of the products of alimentation; for a certain fulness of the bloodvessels is clearly necessary to such brain phenomena as consciousness, as may be inferred from fainting; to excretion, as may be seen by the increase of urine after ingestion of fluids; to general innervation, by the sense of *malaise* which accompanies feeble, and of *bienaise* which is found with proper, heart's action; and a certain frequency and duration of exposure of the blood in respiration, is necessary to the proper interchange of gases, whilst a due amount of action of the heart, as well as of inspiratory effort, is required for the free circulation of the blood through the lungs. Hence, with weakening of the force of the heart, the muscular and nervous forces are weakened, and all vital processes are inadequate to health.

Such a state is that of general debility, deficient innervation, or exhaustion from numerous causes, temporary debility from over-exertion or anxiety, convalescence from almost all diseases, and the state of exhaustion in fevers, with certain limitations.

If, in such conditions, alcohol be administered in such a manner as not to induce derangement of the system, it is easy to see how Dr. Todd's statement may be verified—viz., that it improves the nutrition of the nerve-structures, and does not produce inflammation; and, in addition, it may be shown that, by its power to increase the force of the circulation, it will prevent or remove the tendency to local con-

gestion of organs, as the lungs and liver, and the results of such congestion—viz., effusion of fluid,—and will improve the whole nutritive and vital powers of the body. This is certainly in accord with universal practice, both in this and former ages, for whilst we may appeal to the great mass of the profession in reference to their experience now, we have on record, in the works of Aretæus, an admirable description of the effects of wine in such conditions in his day, which we will transcribe. In the treatment of synope, he remarks: "But if converted into syncope, and this also happens, (the powers of life being loosened, the patient being melted in sweat, and all the humours being determined outwardly, the strength and pneuma being also dissolved,) we must disregard the delirium, and be upon our guard lest the patient be resolved into vapour and humidity. Then the only support is wine, to nourish quickly by its substance, and to penetrate everywhere, even to the extremities; to add tone to tone; to rouse the torpid pneuma, warm that which is cold, brace what is relaxed, restrain those portions which are flowing outwards,—wine being sweet to the sense of smell, so as to impart pleasure; powerful to confirm the strength for life, and most excellent to soothe the mind in delirium. Wine, when drunk, accomplishes all these good purposes, for they become composed by the soothing of their minds, are spontaneously nourished to strength, and are inspired with pleasure."

But we require at least one other indication as our guide in the administration of alcohol—viz., the condition of the skin. The regulation of the heat of the body, as well as the due distribution of the volume of the blood to the central and peripheral organs, rests very much with the skin. We know well the great benefit which results in the last stage of fevers, in local inflammation, and in sun-stroke, from the free use of cold water to the skin, although that could cool the body only by contact and radiation; and of the application of warmer fluids in similar conditions, which, so far as they were of lower temperature than the skin, directly cooled the body, and so far as they approached or exceeded that of the skin, might tend to restore the function of evaporation to the skin, and thus indirectly cool the body. Both these plans are of well-known efficacy, and although apparently opposed, produce, or tend to produce, by different methods, the one result. Now although there may be deficiency in the heart's action, if the skin be hot and dry, it is manifest that great risk may follow the use of alcohols; and when, as in the sthenic stage of fevers and inflammation, the force of the circulation is increased, from the resistance to the current of blood offered by the capillaries in the great inactive external organ of the body—the skin,—it must tend directly to do injury. Hence, it was never the practice to give these substances in the active and hot stage of fever; but when the condition arrived in which the skin had lost something of its dryness, and become soft, and particularly when it perspired, alcohols and bark became the sheet-anchor of the practitioner. With a perspiring skin, wine and other

alcohols could be given profusely, and I once gave six bottles of port-wine to a female in forty-eight hours, in such a condition occurring in fever. These are the states in cholera and in diphtheria in which very large quantities have been administered, with the effect of saving life from imminent peril.

Such, also, were the views of the ancients, for Aretæus again writes; "But if the period be already come to a crisis, if there be dew on the clavicle and forehead, the extremities cold, the pulse very small and frequent, or if creeping and feeble in tone, the patient must take a little food, and partake of wine effectually." And again: "But if much sweat flow, the pulse come to a stop, the voice become sharp, and the breast lose its heat, we are to give as much wine as the patient can drink. For those who are cold, wine is the only hope of life."

Hence, a feeble pulse and an active skin are, and have always been regarded as, the clear indications of the requirement of alcohol, and as the skin loses its excess of action, the rapidity of the pulse will decline, and its fulness and force increase. This is the most perfectly exemplified in the case of many Europeans, even ladies, living in India, who find it really impossible to sustain innervation without the use of some forms of alcohol.

I have before referred to Dr. Todd's practice of giving acetate of ammonia, and may remark, that in cases of fever, when the exhaustion had just begun, it was always the rule amongst practitioners to give ammonia, and not an acid, with the cinchona, with or without wine; and no doubt the true action of this was, to temper the effect of the bark and the alcohol upon the skin, so as to prevent a too rapid reduction of the action of the latter organ. It is, however, singular, that patients with hot and dry skin should have borne the exhibition of twelve or twenty ounces, and even much more, of brandy daily for weeks together, whatever may be the truth as to the mortality, or prolonged duration of the disease, which followed its use. We see, in health, that there is a remarkable variation in the toleration of the system to the action of alcohol, and in certain conditions of disease, we know that the toleration is much greater. If Dr. Todd's statement be correct as to the non-emission of the fumes of alcohol by the breath in certain cases in which large quantities have been given, it is quite open to question if, in such cases, the alcohol obtained its usual entrance into the circulation. This seems to be the explanation of the similar condition in cholera, where the vital power of absorption is so greatly reduced, and it is quite certain that the body has naturally a power to limit the introduction of food and water into the blood, but allows it to pass off by the bowel. This is, probably, the safety-valve provided for such emergencies.

It has fallen to my lot to see cases of subacute inflammation of the lungs, and of the deposit which has succeeded to the inflammation, treated on Dr. Todd's method with alcohol, whilst the action of the skin was deficient, and I certainly have been impressed with the

belief, that the progress of such cases towards recovery was greatly retarded.

The action of alcohol in restraining the secretion of the saliva and of the mucous membranes did not appear to me, in such acute or subacute cases to be of great importance; for whatever might be the state of the tongue, if the skin were freely acting, and other conditions called for the use of alcohol, the alcohol was well borne.

It is impossible to pass over in silence the influence of alcohol in lessening the excretion of urea; for if it do so simply by causing the retention of fluid in the body, it may be most prejudicial in cases of fever, where the whole safety of the patient seems to depend upon the free elimination of the excessive amount of urea which then occurs. It is only when the amount of urea has naturally fallen with the subsidence of the febrile condition that alcohol can be safely administered.

But in any condition in which the amount of urea is lessened by reason of interference with the digestion and transformation of food, it is allowed by all that alcohol is injurious. In this condition it is very likely that the diminution in the flow of saliva may play a very important part, for it must greatly lessen the transformation of starch, and the retention of the fæces with other co-ordinate actions will explain the temporary condition of the liver and the headache, which are their constant attendants.

The efficacy of alcohol in delirium and coma has been quoted as a prime argument in support of recent views. When the delirium is not the result of blood or brain disease, but indicates exhaustion, as in delirium tremens, syncope, or after fright, it is easy to explain that alcohol, opium, or bark, by giving force to the heart, and lessening the action of the skin and the kidneys, is very likely to cause that condition of fulness of the bloodvessels of the brain which is necessary to perfect consciousness. In certain forms of coma the beneficial action of alcohol may be explained by its power to quicken the circulation. In both of these states we shall doubtless find that any remedy which acts in the manner now indicated will be equally beneficial with alcohol. Whilst there is so much diversity and apparent opposition in the remedies employed by various practitioners, there can be no doubt that they have a unity in the mode of their action which it is most desirable to discover.

The local action of alcohols upon the stomach does not call for remark, since the direction of their action is evident and well understood. The statement that alcohol must impede digestion, because when a substance is placed in it, it becomes hard, can have no reference to the action of the moderate and diluted doses of alcohols which are usually employed in medicine.

The frequency and regularity with which alcohol should be given, when it is necessary to prevent exhaustion, have been well stated by Dr. Todd. As the primary action is temporary and passes away in half an hour, the call for it is quickly renewed, and in cases of imminent danger an interval of half an hour would be long enough.

As to the dose, I venture to recommend the use of small doses—say half an ounce of alcohol where spirits would be given; for we have shown that the frequent repetition of a small dose conduces both to a full and a sustained action. It may be well to remember that, as spirits contain from 40 to 70 per cent. of alcohol, sherry and port wines from 17 to 24 per cent., and good ales from 6 to 10 per cent., two ounces of spirit are equal in alcohol to about six or eight ounces of wine and twelve to sixteen ounces of ale.

In conclusion, I would invite attention to the use of the aromas of wines and the saccharine and nitrogenous elements of ales. It is clear that these have an action apart, and sometimes different, from the alcohol, and therefore are fitted for different conditions. The conservative influence of the aromas of wines seems especially to be fitted for the cases of excessive nervous action, in which to reduce the action would conduce to health; whilst in fever it has been remarked that the newer wines, containing little aroma and more spirit than old wines, have a better action.

It is well known that a quantity of alcohol may be taken in wine, and particularly in good wine, in health, without affecting the sensorium, which could not be borne at all if drunk as spirits and water—a fact due partly to the greater length of time over which the drinking of the wine is extended, and in other part to the opposing effect of the aromas. The value of the aromas has been recognised in all ages, and Aretæus directs that "the wine is to be fragrant and not very astringent."

As sugar largely increases the evolution of carbonic acid, and gluten has a similar but less and less rapid action, and in the latter case at least the carbon could not have proceeded from the gluten, I am led to believe that beers have the power of directly promoting the assimilation of carbonaceous food. Whether this is due to the quality of a ferment which nitrogenous matters possess is not here important to discuss; but the combination of alcohol with these principles and with a bitter property gives the most admirable compound which the world has seen, and especially fitted for the cases in which there are general enervation, defective powers of assimilation, and active state of the skin, as in the lassitude and exhaustion of many in hot climates.

Hence there are certain cases in which alcohol is chiefly called for, and in which spirits of wine may be given quite as usefully as the ordinary compounds; others in which the aromas of fine old wines are of great value; and a third class in which the addition of the nitrogenous digestive agent in ales should be given.

I think it would be to the advantage of our patients if, in cases in which we desire the effect of alcohol, and not the other ingredients found in alcohols—viz., increase in the action of the heart and decrease in the action of the skin—we administered spirits of wine instead of spirits, because by obtaining it during the first hours of distillation we may obtain nearly a pure alcohol, and by indicating the

specific gravity we may at all times know the true amount of the agent which we would employ. Alcohol varies very greatly in flavour and degrees of purity, and it commonly happens that the kind which is used in the manufacture of spirits is that which passes of in the later hours of distillation, and contains much fusil oil and free acid, which are very injurious to health. Moreover, as there is a clear difference in the action of alcohol and fine old brandy, we have in alcohol an agent which will effect the two purposes above mentioned, and will at the same time support the respiratory process.—*Lancet, March* 2, 1861, *p.* 212.

149.—ON THE PATHOLOGICAL HISTOLOGY OF TUBERCULOSIS.

By Professor VIRCHOW.

[The following is an extract from "Virchow's Cellular Pathology," a valuable work lately translated by Dr. FRANK CHANCE. The Reviewer of this work in the Dublin Quarterly Journal says:

"Virchow's views with respect to the microscopical and pathological nature of tubercle, are deserving of especial notice; for who has not had reason to mourn from the ravages of this inexplicable disease? and where the physician, so proud of his own attainments, who has not felt the blush of shame mantling his cheek, as the slow and insidious step of this deadly affection stole silently, but slowly, on, and he was powerless in checking its onward progress? Before this unsparing and relentless foe, that mocks its intended victim, while its remorseless gripe is gradually tightening on the well-springs of life, how lowly we fall in our own estimation, wringing from us the sincere, but bitter confession that, in this case, all our art and all our knowledge stand forth like pigmy dwarfs against such a gigantic and invincible antagonist. The fate of the French medical student, who, made aware of this incurable affection under which he was labouring, dared to anticipate by more speedy means, the event which he knew must be his lot, though by a process slower, indeed, but equally as sure, has branded, in characters that never can be effaced, our utter impotency to deal with this destructive malady successfully in the present condition of our knowledge. We ought, therefore, to hail as it deserves, every attempt that may be made to give us a proper insight into this hitherto unfathomable secret, as it may thus conduct us perhaps to a more rational mode of encountering an enemy who has hitherto proved so totally unassailable in every respect."]

It may, at least according to what I consider to be the correct view of the matter, certainly be said, that the greatest part of whatever, in the course of tuberculosis does not appear in the form of granules,

is an inspissated inflammatory product, and has at any rate no direct relation to tubercle. But by the side of these inflammatory products, or also independently of them, we find a peculiar structure [the knot, granule] which, if they are to be regarded as real tubercle, would no longer be included in their ordinary classification; and it is certainly an extremely characteristic circumstance that in France, where the terminology of Lebert has become the prevailing one, and the *corpuscules tuberculeux* are wont to be regarded as the necessary accompaniments of tuberculosis—bodies, concerning the tuberculous nature of which there can be no doubt, have quite recently been set down as something altogether peculiar, and which had hitherto remained undescribed. For one of the best, nay, perhaps the best, micrographer France possesses, Robin, has, in his examinations of cases of tubercular meningitis, deemed it impossible to regard the little granules in the arachnoid [pia mater] which everybody looks upon as tubercles, as being really tubercles, because the dogma now prevails in France that the tubercle consists of solid non-cellular corpuscles, and, in the tubercles of the cerebral membrane, cells in a state of perfect preservation are met with. To such curious aberrations does this track lead, that one ends by being unable to find a name for real tubercle, because so many accidental objects have been confounded with it, that what was sought for, or even what had been found and was already grasped, has, in consequence of the attention of observers being diverted by these objects, being allowed to slip out of one's hand again. I am of opinion that a tubercle is a granule, or a knot, and that this knot constitutes a new formation, and indeed one which, from the time of its earliest development, is necessarily of a cellular nature, and, generally just like all other new formations, has its origin in connective tissue, and which, when it has reached a certain degree of development, constitutes a minute knot within this tissue, that, when it is at the surface, projects in the form of a little protuberance, and consists throughout its whole mass of small uni- or multi-nuclear cells. What especially characterises this formation is the circumstance, that it is extremely rich in nuclei, so that when it is examined as it lies imbedded in the tissue which invests it, at the first glance there seems to be scarcely anything else than nuclei. But upon isolating the constituents of the mass, either very small cells provided with one nucleus are obtained—and these are often so small that the membrane closely invests the nucleus—or larger cells with a manifold division of the nuclei, so that from twelve to twenty-four or thirty are contained in one cell, in which case, however, the nuclei are always small, and have a homogeneous and somewhat shining appearance.

This structure, which in its development is comparatively most nearly related to pus, inasmuch as it has the smallest nuclei and relatively the smallest cells, is distinguished from all the more highly organised forms of cancer, cancroid and sarcoma, by the circumstance,

that these contain large, voluminous, nay often gigantic corpuscles with highly developed nuclei and nucleoli. Tubercle, on the contrary, is always a pitiful production, a new-formation from its very outset miserable. From its very commencement it is, like other new-formations, not unfrequently pervaded by vessels, but when it enlarges, its many little cells throng so closely together, that the vessels gradually become completely impervious, and only the larger ones, which merely traverse the tubercle, remain intact. Generally fatty degeneration sets in very early in the centre of the knot (granule), where the oldest cells lie, but usually does not become complete. Then every trace of fluid disappears, the corpuscles begin to shrivel, the centre becomes yellow and opaque, and a yellowish spot is seen in the middle of the grey translucent granule. This is the commencement of the *cheesy metamorphosis* which subsequently characterizes the tubercle. This change advances from cell to cell farther and farther outwards, and it not unfrequently happens that the whole granule is gradually involved in it.

Now, the reason why I think that the name of tubercle must be specially retained for this formation, as being extremely characteristic of it, is this—that the tubercle-granule never attains any considerable size, and that a tuber never arises out of it. Those which are wont to be termed large tubercles, and attain the size of a walnut, or a Borsdorf apple, as for example in the brain—those are not simple tubercles. You will generally find the tubercles in the brain described as being solitary, but they are not simple bodies; every such mass (tuber) which is as large as an apple, or even not larger than a walnut, contains many thousands of tubercles; it is quite a nest of them, which enlarges, not by the growth of the original focus (granule), but rather by the continual formation and adjunction of new foci (granules) at its circumference. If we examine one of these perfectly yellowish white, dry cheesy tubera, we find immediately surrounding it a soft, vascular layer which marks it off from the adjoining cerebral substance—a closely investing areola of connective tissue and vessels. In this layer lie the small, young granules, now in greater, now in less, number. They establish themselves externally [to the previously existing ones], and the large tuber grows by the continual apposition of new granules (tubercles), of which every one singly becomes cheesy; the whole mass, therefore, cannot in its entirety be regarded as a simple tubercle. The tubercles themselves remain really minute, or as we are wont to say, *miliary*. Even when on the pleura, by the side of quite small granules, large yellow plates, looking as if they were deposited upon the surface, are met with; these too are not simple tubercles, but masses composed of a large aggregate of originally separate granules.

Here you see, form and nature are in reality inseparably connected. The form is produced by the growth of the tubercle from single cells of connective tissue, by the degenerative proliferation of single groups of connective-tissue corpuscles. Thus, without more ado, it appears

at once in the shape of a granule. As soon as it has once attained a certain size, as soon as the generations of new corpuscles which develop themselves out of the old histological elements by a continual succession of divisions, at last lie so close to one another, as to cause a mutual arrest of development, gradually to induce the disappearance of the vessels of the tubercle, and thereby to cut off their own supplies, then they begin to break up, they die away, and nothing remains behind but débris—shrunken, disintegrated, cheesy material.

The cheesy transformation is the regular termination of tubercle; but, on the one hand, it is not the necessary one, inasmuch as there are rare cases, in which tubercles, in consequence of their undergoing a complete fatty metamorphosis, become capable of reabsorption; and, on the other hand, the same cheesy metamorphosis befalls other kinds of cellular new-formations; for pus may become cheesy, and likewise cancer and sarcoma. This metamorphosis, therefore, being common to more than one formation, cannot well be set down as a criterion for the diagnosis of any particular structure, such as tubercle; on the contrary, there are, certain stages in its retrograde metamorphosis, where one cannot help confessing that it is not always possible to come to a decision. If a lung be laid before you with cheesy masses scattered through it, and you are asked if that be tubercle or no, you will frequently be unable to say with certainty what the individual masses originally were. There are periods in the course of development when that which is inflammatory and that which is tuberculous can with precision be distinguished from one another; but, at last, there comes a time, when both products become confounded, and when, if one does not know how the whole arose, no opinion can any longer be formed as to what its nature is. In the midst of cancerous masses also cheesy spots occur which look exactly like tubercles. I have demonstrated that it is by the gradual transformation of the elements of cancer that this cheesy matter is produced. But if we did not positively know from the history of their development that cancer-cells disintegrate step by step, and that no tubercles form in the middle of cancer, we should in many cases be altogether unable to arrive at any decision from merely examining the specimen.

If these difficulties be surmounted which lie in the external appearance of the formation, and lead the observer astray not only when he considers its grosser features, but also when he investigates its more intimate composition, there remains nothing else to assist us in coming to a right conclusion than the investigation of the type of development displayed by the individual new-formations during the stages of their actual development, not during those of their retrograde metamorphosis. The nature of tubercle cannot be studied after the period when it becomes cheesy, for from that time its history is identical with the history of pus which is become cheesy; an earlier period must be chosen when it is really engaged in proliferation. So

in the case of other formations, that period must be studied which is comprised between their origin and their culminating point, and we must see with what normal physiological types they agree. Then it is, I think, certainly possible for us to arrive at a just conclusion with the aid of the simple principles of histological classification, which I have already propounded to you. Heterologous tissues also have physiological types.—*Dublin Quarterly Journal, Feb.* 1861, *p.* 201.

150.—ON BRIGHT'S DISEASE OF THE KIDNEY.

By Dr. S. J. Goodfellow, Physician to and Lecturer on Medicine at the Middlesex Hospital.

[Very different opinions are entertained by pathologists as to the nature of the changes which lead to the formation of the small, hard, contracted form of kidney. Some regard it as a simple degeneration, others as the result of a low and chronic process of inflammation. Dr. Goodfellow believes there is a combination of the two in most cases.]

In this form of kidney there is a great increase of the connective tissue, and not only an increase, but an alteration of the tissue. It is coarser, and is sometimes, I believe, more or less mixed with fibres of the yellow fibrous element. The normal connective tissue, or matrix of the kidney, is extremely delicate, and this, I suppose, has led some histologists to deny its existence altogether. It is very easily destroyed, and it is so extremely transparent that unless considerable care is taken in the management of the light in looking at specimens of healthy kidney tissue as transparent objects, and, therefore, by transmitted light, all appearance of fibres will be lost.

The atrophy and destruction, partial or complete, of the Malpighian bodies and convoluted tubes, which produce the contracted state of the organ, is, in a great measure, owing to this increase and alteration of the fibrous elements. Not altogether, however; for it is probable that, for the reasons assigned in my last lecture, there is, at the same time, and from the same cause, a degeneration and an actual wasting of these structures, independently of the mechanical effects of this increase of fibrous tissue. It is more than probable, as I there hinted, that the delicate anatomical elements composing the walls of the capillary blood-vessels, both Malpighian and secondary, and of the uriniferous tubes, are directly injured by the irritating influence of the alcohol, and other allied substances. This is not only the case with the anatomical elements of the kidney, but probably with those of the liver and other organs also, in which this condition is usually observed. This being probably the case, the question naturally arises, To what is this increase of the fibrous element due? Is it simple hypertrophy of the matrix from increase of normal nutrition, in consequence of the constant state of congestion and blood-delay, due to the influence of

the several causes which I have named as instrumental in the production of this form of kidney? Nutritive hypertrophy it may be called. Or is it from fibrillation and organisation of an exudate, the result of a low and very chronic inflammatory process? Or, lastly, is it an intermediate process, partaking of the characters of both—development by cell-formation and fibrillation? The blastema for this growth of fibrous tissue we have seldom, if ever, an opportunity of seeing in the kidney, for, as I have said before, it is only after the tissue has been already formed, and the kidney has been considerably atrophied, and its secreting structures nearly gone, that death enables us to see the real condition of the organ. But, although we do not see it in the parenchyma of organs, we do on serous surfaces; and, supposing that it does not exactly resemble that, in consequence of the different size and arrangements of the capillary blood-vessels, and the greater intensity generally of the inflammation, and other circumstances, yet it may so nearly resemble it as to enable us to form a notion of the process by which a plastic fibrous exudate becomes converted into fibrous tissue. In the kidney it may not always result from a slow but continuous inflammatory process. It may be from frequent exacerbations, or from separate and frequent subacute attacks of inflammation, owing to the more intensified action of the different causes at different times, when the exuded fibrinous blastema may undergo those changes which we observe in similar exudates on serous surfaces. It may, at times, be partially absorbed, previously undergoing liquefaction by the more fluid portions of the serum, or it may be developed into an organised fibrous structure, either by cell development and growth, or, which is more frequently the case, by conversion into fibrous tissue directly by the dissilience of the fibrillated blastema.

This increase having taken place. How is it likely to affect the true secreting structures? It produces it in two ways, both mechanical in their operation.—First, by pressure upon them, leading to absorption, and secondly, by pressure upon, and obliteration, partial or complete, of the blood-vessels, and in this way cutting off the supply of nutritive blood-plasma. This interception of blood-plasma may probably be induced in another way,—by the peculiar mode of action of the causes of this form of kidney disease, either from actual coagulation of the albumen and fibrine in the blood-vessels, or from the mechanical influence of the separated or precipitated fats in a non-saponifiable form.

But, as I said before, I do not think that the atrophy and destruction of the true gland structures are entirely due to these mechanical influences, certainly not, when the cause is alcohol. I repeat that the mere contact for any great length of time of an irritant with the delicate gland-tissues will lead, under certain conditions, to their degeneration and ultimate destruction. The mere contact, more or less continuous, of an irritant, such as alcohol, in the form of brandy, gin, and such ardent spirits, will produce something resembling a shock

upon the tissues, alter or annihilate their inherent properties, impair or destroy the play of their normal chemical affinities, and consequently deprive them of their assimilating powers. They die, and cease to be renewed, from inanition, much in the same way as they lose the same properties of developement, growth, and conservation, from general impairment of all the functions of the body in old age.

On the Fatty Kidney, when occurring as an Independent Form.—With regard to the process by which this independent form of Bright's disease is produced very little is positively known. It seems to me that the only difference between this independent form, and that which accompanies, and constitutes a part of, the other forms, consists in the fact, that as in these two forms—the large white, and the small and contracted—it is a conversion or metamorphosis of abnormal blastemata and subsequent tissue-formation into fat, so this is a conversion or metamorphosis of a blastema, degraded not by inflammation, or any palpable form of disease, but by some other condition which interferes with the nutrition of the blood, or with the proper development and growth, and healthy changes of the various proximate principles. Instead of the materials derived from digestion being converted into perfect assimilable protein principles, that process of conversion seems to be arrested, and they remain in the form of fatty matters, which, being the only, or at least, the most abundant produce of the digestive process, ultimately replaces the normal protein-tissue. But, besides this, there is strong evidence that, under certain conditions, protein-tissues may, by some process, even in the living body, be converted into fat. There is, probably, another source from whence fatty matters may be obtained. The effete matters, resulting from the waste of the tissues, may not be converted into such oxidisable states as are capable of being eliminated in the form of the perfect excreta of the body, and they may be metamorphosed into fat. The quantity of food daily taken, far too great for the wants of the system, will take away a great part of the oxygen, derived from respiration, that ought to, and would otherwise, have been exclusively appropriated to the oxidation of these effete matters; and if the supply of oxygen is still further diminished by insufficient exercise, or by confinement to an impure atmosphere, the tendency to this fatty production in the body will be proportionably increased. We should, consequently, expect to find this superabundance of fatty matters in the body, and the tendency to the replacement of protein principles by fat in elderly people, in whom the respiratory changes are deficient, who take but little exercise, and take more food than they can possibly convert and assimilate into the higher proximate principles and protein-tissues. In drunkards this conversion probably is more nearly allied to a form of inflammation, and owes a complex causation. As I have already stated, alcoholic drinks are a much more frequent cause of this fatty kidney. It is impossible that the separation of fatty principles, which was observed by MM. Lallemand, Perrin, and Durey, after the admin-

istration of alcoholic compounds, can take place in the blood without obstructing the circulation through the minute capillary blood-vessels, interfering with the transudation of blood-plasma through the interstices of their walls, and leading to a superabundant admixture of these fatty principles with the plasma itself, even if it were otherwise normal in composition, which it very probably is not. How can it, indeed, remain in the normal state? This separated fat most probably comes chiefly, if not entirely, from the red blood-corpuscles, if it be true, as Lehmann asserts, that the fats of the blood are principally deposited in them. It is not surprising, then, that tissues, whether composed of fibres, molecules, or cells, or of all of them, should undergo changes, which ultimately end in fat. Old age, too much food, too little exercise, residence in an impure atmosphere, but above all, an intemperate use of ardent spirits, one, or more, or all, are the true causes of the fatty form of kidney when occurring as an independent form of Bright's disease, and also, in great part, when it is an engraftment upon the others.

On the Amyloid, Lardaceous, or Waxy Kidney, when occurring as an Independent Form.—Virchow was the first to discover the true nature of the metamorphosis that the tissues undergo in this condition of the organ. Those parts of the kidney, and of other organs which have undergone this metamorphosis seem to be converted into a substance analogous in its reactions with iodine and sulphuric acid to substances of the amylaceous group. On brushing over parts affected with this metamorphosis, they assume in a few minutes a deep red-brown colour. This seems to be distinctive, for it is very different from the colour produced by iodine on organs in any other condition, and when once seen, is represented by Dr. Harris not to be mistaken. It is not cellulose, for iodine does not produce the red-brown colour with this substance, and it offers less resistance to alkalies, and is convertible into sugar, which the amyloid substance is not. For the same reason it is not actual starch. The reactions with cholesterine with the same agents, for which it might otherwise have been mistaken, are essentially different. It is therefore not allied, probably, in any way to this substance. The process by which the presence of this amyloid substance may be detected, is very simple, and of easy application. "When a solution of iodine is brushed over a liver which has undergone this change, the affected parts in a few minutes assume a deep red-brown colour, very different, as before stated, from the colour produced by iodine on organs in any other condition. When to these parts, thus reddened by iodine, sulphuric acid is added, a change to a bluish-red or violet-red, or deep blue purple, or even to an indigo-black, speedily commences; in some cases this colour quickly passes into a deep reddish-brown. In the malpighian bodies, and arteries of the kidney, the bluish coloration is most marked, and in these the dilute acid is sufficient to produce it. In the liver the stronger acid is necessary, and the colour is observed with greater difficulty. Now,

cholesterine, when treated with strong sulphuric acid and iodine, shows a very similar blue colour; but with iodine alone it is unchanged in colour. It is necessary, in order that the iodine shall produce its characteristic blue colour, that it undergo some amount of oxidation by the sulphuric acid." The following are Virchow's views as to the nature of the substance, its anatomical seat, and the character of the constitutional symptoms as quoted by Dr. Harris, from his work on "Cellular Pathology":—

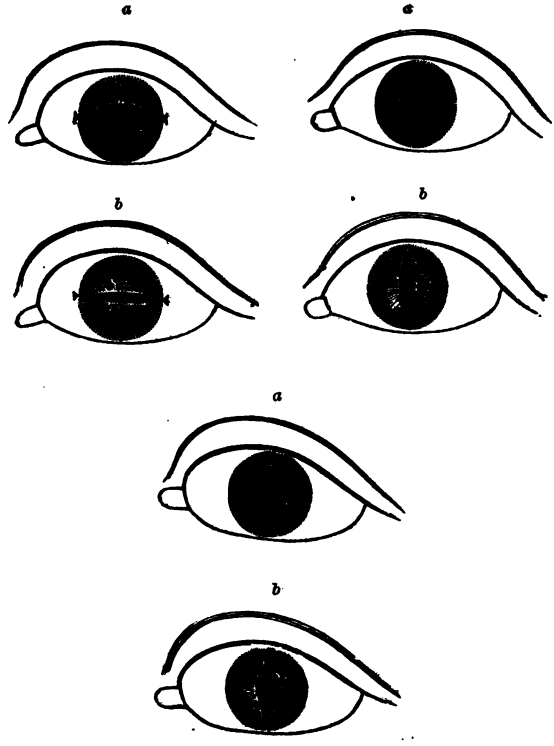
"Almost all parts of the body are capable of undergoing this process of degeneration. The affected parts become enlarged, somewhat indurated and anæmic; the cut surface is semi-transparent, but dull; the natural colour of the parts is lost, but the colour of the neighbouring parts and vessels being seen through, gives them a yellowish or brownish tinge. The coats of the small arteries are the most frequent primary seat of this infiltration, and from them it spreads to the parenchyma of the organs; the walls of the arteries became thickened, and their calibre reduced, and hence the anæmic condition of the organs. The muscular fibres of the middle coat are the parts first affected. In the place of each muscular cell a compact homogeneous body is seen, in which, in the earlier stages, the centre of the nucleus appears as a hole; this afterwards disappears, so that a kind of spindle-shaped body remains, from which all trace of cell-structure has vanished, no distinction being left between cell-wall, contents, and nucleus. When the infiltration has reached this point, it commences to invade the parenchyma of the organs. In the liver, the cells in the immediate neighbourhood of the hepatic arteries are first affected; the liver-cells gradually become homogeneous; nucleus and cell-well gradually disappear; and at last nothing is left but an absolutely homogeneous shining body; the cells are thus converted into a kind of corpora amylacea. In the kidney, the vessels of the Malpighian bodies and the afferent arteries first undergo this change. In the earlier stages but little alteration is perceptible to the naked eye; the kidney appears merely indurated, and anæmic, and only when a solution of iodine has been applied to it, does the change it has undergone become apparent; then, throughout the cortex numerous fine red dots appear, corresponding in their size and position to the glomeruli, and fine red streaks running from them, indicate the afferent arteries.

"The disease is constitutional; one organ alone is rarely affected; the only spot, where as yet an independent development of this change has been remarked, was in the permanent cartilages. The organs thus affected, cease to discharge their functions; the patients assume a cachectic appearance, and gradually waste away; dropsy frequently supervenes. Sometimes, too, the whole digestive tract is affected by this degeneration. During life this is rendered manifest by continued diarrhœa, and by diminished powers of absorption."—*Med. Times and Gazette, Dec.* 15, 1860, *p.* 573.

151.—MR. BOWMAN ON CONICAL CORNEA.

[Having been favoured by the Editors of the Ophthalmic Hospital Reports with the wood-cuts illustrating Mr. Bowman's paper at page 258, but which were received after that article was printed off, we here append them. Mr. Bowman says:]

It is interesting to observe the movements of the pupils after these operations. The following diagrams exhibit the states of dilatation and contraction in the case of horizontal slit, of vertical slit, and of the balloon-shaped pupil from iridesis downwards; *a*, dilatation; *b*, contraction.



Ophthalmic Hospital Reports, Oct. 1859, *p.* 165.

The second volume of the "Transactions of the Obstetrical Society" contains numerous valuable papers, some of which we here bring before the practitioners of midwifery. We received it too late to enable us to get the papers ready for insertion in their proper place.

152.—ON A SPECIAL POSITION IN LABOUR.

By ROBERT HARDY, Esq., Lecturer on Obstetrics to the Hull and East Riding School of Medicine.

[We have often found the following practice of Mr. Hardy's exceedingly useful. It was long ago suggested by Mr. Craven, of Hull, that in a case of lingering or impeded labour, the woman should alter her position, and *sit upright between two chairs* till the head was nearly born, the patient being then removed to bed. Mr. Hardy publishes a most interesting paper confirming this practice of Mr. Craven's, and illustrates it by several engravings in the Obstetrical Transactions, showing the various axes of the pelvic canal. Mr. Hardy explains his views as follows:]

The placing a parturient female in the sitting posture may be effected in two ways—1, by seating her between two chairs, suitably arranged; or 2, by adopting a simple and inexpensive apparatus made for the purpose. In private practice the chairs are most convenient, and answer admirably; whilst in obstetric institutions perforated seats on firm supports, and with side arms, are to be preferred.

The position on the chairs may be explained thus: Two firm chairs, of equal height, are secured to each other *in front*, by tape or other strong material; their *backs* are then separated from each other one and a half or two feet, according to the breadth of the patient's nates. The chairs, to secure them from moisture, as also to save the patient's thighs from painful pressure, are covered by folded napkins; and it is further desirable that they be placed on the *left* side of the bed, *facing the footpost*, in order that the patient, at the proper time, may be quickly removed on to the bed.

The woman's assent to the measure having been obtained, and the obstetric binder carefully secured round the whole of her abdomen, she is removed from the bed, and *seats herself well over or between the chairs*. When doing this, the assurance is given her that no exposure of her person, while thus situated, can possibly occur. Standing behind the chairs, the accoucheur is to take care that the patient's *dress* is *free* and well drawn down; that her *nates* are projected a little *below* the plane of the chairs; that her *knees* are somewhat separated and pressed firmly against the framework of the bed, and her *feet* resting on a firm support.

Thus circumstanced, the woman is to bend forward and grasp firmly the footpost of the bed; should there be none, then the waist of an attendant, seated on the *side* of the bed, must be substituted for the

same. The accoucheur, situated *behind his patient*, seats himself on a low chair or stool, or, if he prefer it, may kneel on a cushion placed on the floor. Thus circumstanced, his occasional examinations of the woman enable him accurately to note the progress of the labour, and at the proper time to have her removed to bed. For the sake of cleanliness, it is proper also to place an ewer beneath the nates, to receive the liquor amnii and contents of the rectum.

When the *perforated seat, with arms*, is the medium adopted, there is less necessity for the bedpost or friend in front, as the arms of the chair are giving firm support to those of the woman during the expulsive efforts.

Having premised thus much relative to the position of the patient, I must now remark that as it is *not* the design, so neither is it necessary or advisable to effect the delivery whilst she is seated *on* the chairs. I would lay great stress on this point, for *two* reasons. The first is, that, thus circumstanced, the perineum cannot be so efficiently guarded from lesion as in bed; and secondly, the infant, when born, cannot be so conveniently received by the attendant in this position of the mother.

The rule, therefore, is, when the difficulty in the labour has been overcome, which is known by the ossa parietalia beginning to advance from behind the perineum, the woman is to be removed forthwith to the bed, and placed in the ordinary parturient position. Some slight cessation from pain sometimes follows this removal, but, as a general rule, the head passes in a few pains, and the delivery is, ere long, effected with perfect safety to both mother and infant.

In *all* cases it is to be understood, before the patient is placed on the chairs, that the presenting part (head or other organ) *is clearly within the pelvic inlet, and the os uteri dilated fully one half;* and further, that in every case a reasonable period has been allowed to the horizontal position, and its *failure* evidenced *ere* the measure is had recourse to.

The contra-indications to the sedentary posture in labour are one or more of the following:—1. *Systemic exhaustion*, or symptoms denoting its near proximity. 2. The existence of *inflammation* in any of the internal viscera, or parts more immediately affected by the labour. 3. *Serious uterine hemorrhage.* 4. *The previous advent of puerperal convulsions.* 5. *Prolapsus of a pulsating funis.* 6. *The presentation of parts at the os uteri requiring version.* And 6. *The presence of extreme pelvic obliquity*, wherever situated.

The modus operandi of the sedentary posture and binder in labour, in so greatly expediting that process, is not inaptly illustrated by *the act of defecation, as performed under circumstances of difficulty and distress*, as, for instance, in cases of *obstinate constipation or dysentery.* In these instances the greatest amount of expulsive muscular effort, voluntary and involuntary, is put forth, of which the system is capable, in order to contract the abdominal space and force down-

wards the irritating alvine contents, first *into*, and then *through*, the pelvic cavity.

Seated on the chairs in the manner described, the same series of voluntary and involuntary muscular actions is induced and maintained as in difficult defecation, in addition to and in union with the powerful periodic and expulsive efforts of the *uterus*. The head and body are brought forward to an angle varying from thirty to forty degrees from the perpendicular, by which action the axes of the pelvic inlet and abdominal space are brought into perfect accord. *The hands, feet, and pelvis are becoming so many fixed points*, which are aiding the system in exerting itself to the very utmost in its expellent efforts. The result of all this is seen in the following important advantages, which are secured to the parturient female, viz.—1. Every available succour arising from *the force of gravitation* in the uterine ovum. 2. The putting forth, under highly favourable circumstances, of the most energetic *muscular efforts* of which the system is capable. 3. The bringing, as before named, the pelvic and abdominal axes into the same plane or line of agreement. And lastly. The great support and energy imparted to the *fundus uteri and abdominal muscles* by the *obstetric binder*, suitably applied.

Of the obstetric binder, not only when used in connection with the sitting posture, but as an independent aid in the treatment of *lingering and impeded labour*, I have formed a much higher estimate than is accorded to it by the profession generally. I am accustomed to make frequent use of it *before* the birth of the infant, where the chairs are *not* called into requisition. I find it greatly excites the laggard and strengthens the enfeebled uterine energies, thereby expediting the progress of the labour to a very useful extent. The good effects of this agent in cases which are simply *lingering* are most admirable. A binder *which embraces the whole abdominal parietes within its grasp*, pinned on firmly and smoothly, imparts wonderful support and energy to the *fundus uteri*, where it is most needed; and the labour, which before its application had remained in nearly *statu quo* for several hours, will frequently be terminated in half an hour, or at most an hour, after its application. In such cases it is important that the uterus should be kept well closed by the hand (through the abdominal walls) for a short period after the uterus has expelled its contents; then a couple of folded napkins should be placed over it, and the binder tightened and pinned securely.

In impeded labours, where the resisting powers to the delivery are not very serious, the application of the binder to the abdomen will frequently suffice for the delivery of the woman. In these cases, the laying hold of the binder by the practitioner's left hand, and firm traction made during each expulsive effort, will most materially aid the delivery; the same precautions being taken after the birth to secure contraction of the uterus, the expulsion of the placenta, and the prevention of post-partum hemorrhage.—*Obstetrical Transactions, Vol. 2, p. 83.*

153.—ON THE TREATMENT OF MENORRHAGIA.

By Dr. C. H. F. Routh, Physician to the Samaritan Free Hospital.

[Dr. Routh has published an interesting paper on this subject, but we cannot say that we approve of his treatment. It is necessary, however, that our professional brethren should judge for themselves, and we therefore give them the views of this physician, as his respectable position must have weight in our estimation of his practice. In one case of menorrhagia, the hemorrhage was stayed by the sponge-tent, but it returned. The uterine mucous membrane was then scraped off with Dr. Simpson's intrument, and tincture of iodine injected. Dr. Routh says:]

This patient I have seen since, and she continues well. The case is one of those obstinate cases of menorrhagia where no polypi exist, but where the blood is apparently secreted from a villous membrane. Remedies, in this case, at most did only temporary good, and the constant recurrence of the flooding was such as to justify alarm in relation to the ultimate issue of the case, if not speedily and permanently arrested.

[In another case of flooding, the womb being dilated and pulled down, a finger was introduced into the cavity and a small polypus removed by a curved gouge, and then tincture of iodine injected without scraping off the mucous membrane. Dr. Routh then proceeds to remark:]

The treatment by injection or by the uterine gouge in menorrhagia, singly, have been in vogue for several years, but neither have received that support from the British profession to which I think they are entitled.

This has arisen, I presume, because the modification of these plans which I now recommend, and which forms the original portion of this communication, was not observed. Again, I do not claim to be even the inventor or author of this modification. The injection of the uterus by astringent liquids, or the scraping of the uterine mucous membrane by the gouge, were, as separate operations, first introduced by my colleague, Dr. Savage, in the Samaritan Free Hospital. His uniform success led me to adopt it also, although I used stronger astringent solutions, and occasionally injected immediately after using the gouge. It is generally believed that to inject within the uterus any astringent injection is calculated to bring about peritonitis, and several cases of death from this cause, as a result of this treatment, have been quoted. It is unnecessary here to cite the authorities for this opinion. But as it expresses the opinion of modern accoucheurs, that of Dr. Simpson may be here given, who, in his late admirable lectures, published in the 'Medical Times and Gazette,' writes as follows:—"But, mark you, never think or dream of throwing liquids into the interior of the uterus by means of any injecting apparatus,

for severe and fatal influences are very likely to ensue." And again: "The consequences of injecting fluids into the cavity of the womb are so often dangerous and deadly, that the practice has now been given up, I believe, by all accoucheurs." I feel very diffident in venturing to controvert so high an opinion. I admit these results, but think they depend not so much on the injection *per se* as upon the *modus operandi*. I think the common supposition that, in these cases, some of the fluid penetrates up through the Fallopian tubes into the peritoneal cavity, is not proved, except a force capable of rupturing the organ itself were used, because, precisely as in the case of the glottis, a contraction of the muscular structure would take place around the tube, and thus no entry of the fluid can occur.

Indeed, Dr. Simpson himself seems to doubt this transmission upwards. When speaking of the fatal results due to injections, he adds —"Such a result *may*, perhaps, be caused by the fluid running through one or other patent Fallopian tube, and escaping into the peritoneum; *more probably*, it may be due to laceration of the mucous membrane, and entrance of the fluid into one or more of the uterine veins." I had an opportunity, however, when in Vienna, of seeing this fact proved by an experiment. A woman, the moment after her death, was brought down into the dead house, the abdomen opened, and the uterus exposed before the irritability of the muscles had been destroyed. Dr. Ciari (the late professor of the Secret Obstetric Department, and one of the physicians to the division for diseases of women in the Allgemeine Krankenhaus) having placed the end of a powerful syringe within the uterus, and secured it there, attempted by main force to force up a quantity of fluid through the Fallopian tubes, and utterly failed; and yet, so far as one could judge from her antecedents, there was no reason to suppose that these were impervious. If the ordinary precautions are employed which are commonly taken when injections are given, no fluid could thus penetrate into the peritoneal cavity. Peritonitis, if it occur, would rather be due to *sympathy* of neighbouring parts or rupture of the uterine surface extending within the veins. But if *sympathy* was the common cause of peritonitis, why should not solids, liquified, as they must be, by the uterine discharges, not give rise to it also, whether when applied to a uterine mucous membrane or an ulcer of cervix? The absorption by a ruptured vessel appears thus more probable. We have an example of this kind of extension in those cases of metritis which occur after labour, and where the inflammation extends to the peritoneum, not through the Fallopian tube on its fimbriated extremity, but in the neighbourhood of the broad ligaments. Again, to set up this inflammation by sympathy, within the uterus, the injection must be more or less retained for a time, or very acrid in kind.

These facts being premised, it follows that, if any injected fluid *is not long retained within* the cavity of the uterus, no bad effect will follow. Experience, however, proves that if, through any untoward

accident, it is so retained, then it will give rise to intense pain, and, as a result, inflammation may be expected to follow.

[It is very likely that Dr. Routh's opinion about the danger of iodine injections is true,—viz., that there is no particular danger if you first fairly dilate the os uteri so as to allow free egress to the solution, but that when you inject tincture of iodine through a narrow cervix uteri, it is retained in utero, and gives rise to mischief. In all kinds of injections, therefore, always see first that the os is properly dilated.]

It would appear that in these obstinate cases of menorrhagia there are two conditions of the uterine mucous membrane which require to be removed; one is the presence of polypi of various sizes and number, which grow from its surface; the other is a softened state of the mucous membrane, probably *inflammatory* in character, and which bleeds upon the slightest touch, in many cases upon the slightest movement of the patient.

I was lately enabled to notice this state of mucous membrane in an old woman in whom menorrhagia was present, and in which there was complete prolapse of the uterus. Dilatation of the os enabled me very readily to see the mucous surface of the uterine cavity, which was covered with villi, varying in size, to the eye, about one-eighth to one-sixth of an inch long. On being touched, these bled profusely. The microscopical characters were those of erectile tissues surrounded by epithelium. It is easy to understand how, with such a structure, hypertrophy of one villus would produce a polypus.

The plan I adopt in these cases is to dilate the uterus by a sponge-tent, after the manner so beautifully described by Dr. Simpson in the Edinburgh Monthly Journal, (vol. for 1850, p. 7). This is consecutively done for three or four days, until at last the cavity is so large as readily to admit the index finger. I then draw down the uterus by means of a peculiarly constructed, corkscrew-shaped hook, which has, I think, a decided advantage over those in ordinary use, neither compressing the cervix, as in the case of tenacula, nor tearing the os, as is so commonly done with ordinary single or double hooks, a result very likely to take place when, by the very dilatation previously practised, the os is thin and easily torn through. Pulling down the uterus by this instrument, and pushing the index finger up into the uterine cavity, I am enabled to feel any irregularities or small polypi on the lining membrane. But in this way polypi may be felt quite at the superior part of the fundus uteri, which, owing to the length of vagina and uterus and the shortness of the finger, might have been overlooked. The finger being now removed, the scraper or gouge is introduced, and the whole mucous membrane removed. It is remarkable how, where the amount of hemorrhage has been so excessive that even the mere touch of the sound has almost justified alarm at the consequences, yet the moment the uterus has been denuded of its mucous membrane all

hemorrhage ceases. This done, about half an ounce of tr. iod. co. or tr. ferr. sesq. is conveyed by means of an elastic-gum catheter to the superior portion of the uterine cavity, and allowed to flow down through a speculum in the vagina. The case is now left to itself; if pain occurs, an opium suppository is applied within the rectum. One application of the injection often suffices, but more lately I have repeated in very bad cases the injection, *i.e.*, from three to five or even six days, so long as the os continues patent enough to admit a goose-quill high up. After the second or third day a reddish, mucous discharge begins to flow, where tr. ferr. sesq. has been used, accompanied with large, thin, black flakes, which look very like scales of charcoal; this is, however, soon replaced by an ordinary leucorrhœa, and the cure is generally complete. The operation may sometimes fail in those cases where the hemorrhage is due to a softened, probably inflamed, villous membrane, if the whole diseased part of the mucous membrane, or perchance some polypus growing upon it, may not have been scraped away. The disease may, therefore, at no distant period, reappear. Now, I believe that in many of these cases the injection, if applied at once, corrects the error of the operator, by constringing and and then setting up a healthy and absorptive action in the lining membrane of the uterus, and particularly in the diseased parts accidentally left behind. Indeed, this absorptive action, it might be said, in some measure was set up by the very sponge-tent itself. I agree upon this point with my colleague, Dr. Savage, that the mere and occasional dilatation by a sponge-tent suffices to cause the absorption and diminution of volume in the uterus, besides increasing the tendency of the uterus, after a time, to contract and expel its contents from the very irritation set up, so far favouring the egress of any fluid which may have been retained or even injected.

It has been asserted the scraping of the uterine mucous membrane may cause sterility. This is not even probable. On the other hand, sterility must certainly exist in cases of menorrhagia. Mucous membranes are readily reproduced, particularly that of the uterus, which is normally thrown off entire as decidua after labour, and again reproduced. Moreover, the catamenia are regularly established after the operation of scraping. This is *prima facie* evidence of possible conception.

The last objection made is that such cases may get well under other general treatment. Granted they do occasionally, but then it is only after many weeks' suffering, perhaps months, where local measures would have effected a cure in a week or ten days. It is equally certain some of these cases will not get well by any other means.

In conclusion, I do not mean to say that the method of cure now recommended will be successful on all occasions, and it may have to be repeated. Nor do I mean to say that in instances where polypi are larger than a small nut, and confined to the cervix, that it is to be practised. Here excision, by Dr. Locock's instrument or the knife, is

the better plan, and in these cases it will be found that the corkscrew hook before mentioned will often suffice to bring it down for excision with ease, or for destruction by potassa fusa. The last effectively prevents a recurrence. The only danger of this last plan is the fear of destroying too deeply, perhaps causing in the sequel obliteration of the os, a result I have seen follow the too free application of lunar caustic, although I am free to admit that, with so skilful an operator as Dr. Simpson, such mishaps could not occur. But we are not all Sir C. Lococks or Dr. Simpsons, and the plan I propose is, I think, less dangerous. In cases where numerous and small polypi are found in the cavity of the uterus and at the fundus, I think the plan now proposed is to be preferred.

The uterus need not be scraped on every occasion. Injections, if preceded by a sponge-tent and repeated frequently, may obviate the necessity of the gouge.

[We have given Dr. Routh's opinion fairly. He is supported in this practice by many of the best practitioners, but nevertheless we, should decidedly object to all this scraping of the internal surface of the womb, because we think that it is unnecessary, as there are few, if any, cases of this description which will not yield to the ordinary methods of treatment, assisted by removal of the polypi, if these exist.]—*Obstetrical Transactions, Vol. 2, p.* 117.

154.—ON PELVIC ABSCESS AFTER LABOUR.

By Dr. George D. Gibb.

[We point out this short paper of Dr. Gibb's, because we suspect that such cases occur in private practice oftener than is suspected. A woman had been confined seven weeks, when a pain commenced in the right iliac groin, which became incessant, and was followed by a swelling above the pubes, accompanied by pain on pressure. Now in such a case, after confinement, always examine carefully all round the vagina for a fluctuating tumour, which will indicate where one of these pelvic abscesses following pelvic cellulitis will point, and where relief may be obtained. Dr. Gibb says:]

7th. On careful examination, I detected a prominent fluctuating tumour but hard and painful, occupying the lower part of the posterior wall of the vagina. Poultices were ordered to this, and an alterative and tonic mixture presented three times a day, each dose containing two grains of iodide of potassium with a little tincture of opium; to have, besides, ten grains of rhubarb twice a day.

11th. Bowels relieved two or three times. The abscess in the vagina had burst, but another had formed around the urethra, which is much more swollen, hard, and inflamed. Numerous small ulcers are present on each side of the labia, from the meatus downwards;

pulse 92, a little stronger. Although these ulcers are not specific, I prescribed lotio nigra and small doses of mercurials and Dover's powders thrice a day.

It is well known that pelvic cellulitis is a serious affection occurring after delivery, and when followed by suppuration some anxiety is felt as to the course the matter may take. If the abscess should burst into one of the outlets of the body, like the uterus and vagina, or the bladder and rectum, there is always a fair prospect of recovery. Fortunately in the present instance, nearly eleven weeks after the first symptoms declared themselves, the abscess pointed at the lower and back part of the vagina, and spontaneously evacuated itself, with complete contraction of its walls. Some matter also found its way to the anterior wall around the urethra, which no doubt was the cause of much of the difficulty of micturition. A question arose in my mind, whether both of these abscesses originated from the same source, that is, the primary suppuration higher up in the right side of the pelvis, and it seemed to me that it must be so, from the fact of their forming pretty much about the same time. The quantity of matter discharged did not exceed several ounces, and to this I attribute the successful result of the case. The general health was well-supported throughout by liberal diet and wine, together with tonic and anodyne remedies. There was a curious feature in the case, which I have not mentioned, and that is, the patient could take nothing warm ; all her food had to be swallowed cold.

The catamenia appeared seventeen weeks after confinement, and subsequently resumed their healthy condition. And what is of some importance in the history of this case, pregnancy again took place, and a second child was born nearly seventeen months after the birth of the first, without any inconvenience or bad symptom whatsoever, although the patient's mother was exceedingly anxious about her.—*Obstetrical Transactions, Vol. 2, p. 326.*

155.—ON ASSALINI'S FORCEPS.

By Dr. A. B. Granville, F.R.S., Vice-President of the Obstetrical Society of London.

[In a long but excellent paper "On certain Phenomena, Facts, and Calculations, respecting Propagation in Females of the Industrious Classes in the Metropolis," Dr. Granville recommends these forceps as follows :]

In an essay on the use of the forceps, with directions for the application of both the short and the long one, which I published in 1820, with a few cases, I strongly recommended, as preferable to all others, what is called Assalini's forceps, which I slightly modified. In my hands it has served both purposes. It is very handy, easier of application, and requires no cross-locking of the blades. It also gives

a greater command over the fœtus to the operator, who is not puzzled with a right and a left blade. It is a curious fact that, up to the date of the publication here referred to, none of the latest writers on midwifery, subsequent to Smellie, had noticed the application of the forceps to the head, after the birth of the body of the child, or given any direction on the subject. Smellie was the first to employ the forceps under such circumstances, and evidently he considers himself to have been singular in that respect, as he says—"These two successful cases gave me great hope that the above method would be of great service." He attended two more cases afterwards. I followed his example, and gave in my essay a full detailed case, out of several, illustrative of the manner in which the application should be accomplished. In that essay I dissent from the ordinary received canons respecting the use of the forceps, which, until lately, had been accepted as infallible, namely, that the head of the child shall have rested on the perinæum for six hours before we thought of applying the forceps, according to Denman, and that the forceps may be applied only when an ear of the child can be felt from the os externum. Both rules are bad, as far as they were intended to be exclusive.—*Obstetrical Transactions, Vol.* 2, *p.* 172.

156.—AN INTRA-UTERINE FIBRO-PLASTIC TUMOUR, REMOVED BY ENUCLEATION.

By Dr. J. Hall Davis, Physician to the Royal Maternity Charity.

[In this case Dr Davis detected a hard circumscribed body which seemed to be the uterus extending itself above the pubes.]

On now examining per vaginam, I found the uterus enlarged, the ordinary projecting vaginal portion of the cervix obliterated, owing to the development which the uterus had passed through for the accommodation of a growing tumour. The orifice of the uterus was open to the size of half-crown, and within it I felt a roundish tumour, smooth, and fibro-fleshy to the touch, if the expression may be used. I found this growth adherent to the anterior walling of the uterus, also to the right walling down to the margin of the orifice. Likewise to the left walling to within an inch of the edge of the uterine orifice. Posteriorly the tumour was quite free, as high as the fundus of the uterus, which the uterine sound informed me was distant from the uterine orifice between four and five inches, in other words, that was the depth of the cavity of the uterus. I could not pass the sound from behind round the tumour at the sides, it being there stopped by the lateral adhesions.

It occurred to me that, if I could get my finger through and within the reflection of mucous membrane passing from the cervix to the inferior surface of the tumour, I might succeed in gradually detaching or enucleating the tumour from below upwards. I proceeded first in

front, and detached the tumour up to a line, beyond which I could carry the separation no further. I then detached the tumour laterally, and here a little higher than in front. My impression was that at least half, if not two-thirds, of the growth was now free and unattached. I now desisted, fearing to do mischief, the remaining attachment being very firm.

Very little hemorrhage attended or followed these proceedings. I washed out the uterus with tepid water. The patient passed a good night, without pain.

I next guided upon my index-finger a pair of vulsellum or Museaux's forceps; with which I got a firm purchase upon the lower part of the tumour. and gradually dragged down the mass, so that the most dependent part (indeed all below the ligature) which was now putrid, flaccid, and very compressible, passed into the vagina. Small portions broke away from time to time, upon which I got a firmer hold higher up. I cut off some portions, and proceeding with my traction, I at length brought the putrid part, which formed nearly two-thirds of the tumour, through the os externum. I now obtained a glimpse of the upper, undecomposed part of the growth. During this time firm pressure was being applied over the fundus of the uterus above the pubes.

The patient now complained much of the traction. I therefore examined and satisfied myself that I was not inverting the uterus. This organ had been brought altogether lower into the pelvis, and after the larger part of the growth had appeared externally the uterus had undergone some contraction of its muscular fibres and of its bulk. I proceeded with my traction, getting a fresh hold higher up, and now had the yet extensive attachment of the tumour under my command, when, by a combination of traction and twisting of the tumour close to its adhesion, I succeeded at length in brushing it away, as it were, cleanly from its connexion to the uterus, and so delivered the patient of her unnatural offspring.

I now washed out the uterus again with the same lotion as before; scarcely any blood came away. Some port wine and water were given during and after the operation, which, owing to the slowness of proceedings that I judged advisable, occupied nearly half-an-hour. I gave no opiates by the mouth, as they always disagree with the patient when so taken. I had a laudanum poultice of linseed meal applied to the hypogastric and sacral regions, where she complained of pain.—*Obstetrical Transactions*, Vol. 2, *p.* 19.

NOTICES OF NEW WORKS
LATELY PUBLISHED.

157.—*On Myalgia, being a Treatise on Painful and other Affections of the Muscular System.* By THOMAS INMAN, M.D. (Lond.) Physician to the Liverpool Royal Infirmary.—*London*, Churchill, 1860. pp. 307.

[We have already given so many papers by Dr. Inman on this subject in several of our volumes, that we need only say here, that this work places before the reader, in a connected form, all the views of this talented physician. It has given us considerable instruction with respect to the subject of myalgia, which is so liable to be mistaken for neuralgia, that we recommend all medical practitioners to make themselves masters of Dr. Inman's views.]

158.—*On Asthma, its Pathology and Treatment.* By HENRY HYDE SALTER, M.D., F.R.S., Assistant-Physician to Charing-Cross Hospital.—*London*, Churchill, 1860. pp. 372.

[We must say of this (as of Dr. Inman's work on Myalgia) that we have already presented our readers with most of Dr. Salter's views on the subject of Asthma, reprinted from the pages of the weekly journals. The present volume embodies the author's views on this most intractable disease. It is the best work on the subject with which we are acquainted, is ably and pleasingly written, and contains the results of a vast amount of original observations, especially on the effects of remedies in this disease.]

159.—*On the Reparative Process in Human Tendons, after Subcutaneous Division for the Cure of Deformities.* By WILLIAM ADAMS, F.R.C.S., Surgeon to the Royal Orthopœdic and Great Northern Hospitals.—*London*, Churchill, 1860. pp. 174.

[This work is illustrated by numerous lithographic plates and woodcuts, and altogether forms a complete treatise on this subject, well worthy of general perusal and study. Mr. Adams' conclusions are, that:

1. Tendon is one the few structures of the body which is reproduced.
2. When a tendon is divided and separated, the interval is filled up by new tendon, which re-unites the divided ends of the old tendon.
3. This is accomplished alike in man and animals.
4. The new tendon is liable to adhere to the neighbouring fibro-cellular tissue, and may thus spoil the cure, without great care.
5. The reparative process may be spoiled by the too early and too rapid mechanical extension afterwards.

6. It is not necessary to approximate the divided ends of the tendon soon after the operation, and then to *stretch* the new growth in order to make it long enough, thus gradually stretching it for a month or six weeks, like a bit of vulcanized india rubber; but, on the contrary, "the required length should be obtained during the time occupied in its formation, *i.e.*, in about two or three weeks." Therefore the object of gradual extension is, "to regulate the length of new tendon, by forcibly overcoming ligamentous resistance in some cases, and by preventing a too rapid and excessive separation of the extremities of the tendon in other cases."

These are the most important and practical conclusions; but Mr. Adams' work itself must be referred to for more information.]

160.—*A Practical Treatise on Diseases of the Skin in Children.* From the French of CAILLAULT. By ROBERT HOWARTH BLAKE, M.R.C.S.—*London*, Churchill, 1861. pp. 277.

[The skin diseases occurring in children are very frequently most obstinate in character, and the reputation of the medical attendant is much injured, if, month after month the little sufferer remain unrelieved, as we have frequently known to be the case. To those in large midwifery practice this little work would be of much service, as it is concise and practical. The following extract on the treatment of Syphilis will probably prove interesting to our readers:]

We have endeavoured to show, contrary to the opinions of many authors, that hereditary syphilis is rare, and that many syphilitic parents generate children not merely free from any syphilitic taint, but perfectly healthy and vigorous. What practitioner has not had patients who were undoubtedly syphilitic, and yet whose children enjoy robust health?

The existence of syphilis, however, being once positively established in a new-born infant, what is the most proper treatment to be adopted? and at what period should it be commenced? These are the first two questions which arise when a case of infantile syphilis occurs.

We have noticed the marked differences which distinguish adult from infantile syphilis; and, notwithstanding the numerous and fruitless attempts to make their stages correspond with each other, it has always been found impossible to arrive at any useful practical results founded on these false analogies. Thus, *à priori*, iodide of potassium would seem to be indicated when the infant exhibits symptoms more or less resembling the tertiary lesions of the adult, and still more so when the syphilitic child is born of parents suffering from the characteristic symptoms of that diathesis.

All these theories are valueless in the face of clinical observation. If we read attentively the numerous cases of recovery from infantile syphilis, and note the curature means which have been employed,

we shall be convinced that in the majority of them the existence of syphilis is more than doubtful; and, further, that the variety of these lesions is so great that the changes they undergo under the influence of therapeutic means will not warrant us in forming an opinion as to the superiority of any remedy with reference to any given symptom.

In accordance with our own observations we think that mercury should be prescribed in the form of proto-iodide; the bichloride requires too much dilution to admit of its administration to new-born children. (p. 40.)

[It was formerly a favourite plan to administer mercury to the infant through the medium of the milk of the mother, or of an animal. This practice is still followed in Paris, where several establishments exist in which various medicaments are introduced with more or less success into the milk of animals. The constitutional effects of mercury are very readily produced in infants by cutaneous friction, but M. Caillault prefers the use of mercurial baths, containing from fifty centigrammes to a gramme, (7—15 grains) of bichloride of mercury. No bad effects result from their use, and their favourable local action in these cases is sometimes quite surprising. M. Guillot, an authority quoted by Caillault,]

Prefers to all other remedies, a julep containing twenty-five milligrams of the protiodide of mercury, a tablespoonful of the liquid, previously shaken, to be given every quarter of an hour. If the medicine gives rise to vomiting, which is by no means unusual, the interval between the doses should be half an hour. If, notwithstanding this precaution, the infant should continue to vomit, or diarrhœa or colic should supervene, the administration of the medicine must be suspended. Contrary to the precepts contained in many books, M. Guillot advises great caution with regard to the doses of protiodide, administered internally, but he recommends us to increase the strength of the mercurial bath, which may be done with safety. (p. 43.)

The questions with regard to the duration of the mercurial treatment and of the most appropriate time for its commencement are not precisely stated by authors. The greater portion of them do not praise any preventative treatment. It seems to us that we should at least wait, before inflicting a mercurial treatment upon a new-born child, until the diagnosis is confirmed by a strict investigation, knowing how frequently healthy children issue from a tainted source. The duration of the treatment should not generally be in proportion to the persistence of the lesions. When the child regains its strength and flesh, when all the functions proceed regularly, it is not advisable to continue the anti-syphilitic agent until the cutaneous lesions disappear, for if administered beyond a certain time, it produces derangements of the bowels which require its discontinuance. The mercurial baths alone may be continued without any inconvenience, taking care to

administer them at shorter or longer intervals, according to the exigencies of particular cases.* (p. 44.)

161.—*A Manual of Minor Surgery and Bandaging.* By CHRISTOPHER HEATH, F.R.C.S., Demonstrator of Anatomy at the Westminster Hospital.—*London*, Churchill, 1861. pp. 208.

[As stated in the preface, the object of this work is to present, in a concise form, those minor points not contained in larger or more systematic works on surgery; yet, which details are inexpressibly useful in practice, and much contribute to success. The work is chiefly intended for the use of house-surgeons and dressers to hospitals. We extract a few of the most useful passages.]

Stabs in the chest may produce hemorrhage by wounding (rarely) an intercostal artery, or the lung. An intercostal artery may be twisted or tied, and seldom gives much trouble. Wound of the lung (generally shown by bloody expectoration) may be irremediably fatal from hemorrhage, the blood either pouring from the mouth or choking the lung, or more rarely filling the pleura and compressing the lung. The great object is the immediate arrest of the hemorrhage, by the inhalation of the vapour of turpentine, sprinkled on a handkerchief, the application of cold both to the surface of the chest and by swallowing ice, and lastly by venesection if necessary. Venesection for this purpose should be performed while the patient is supported in the erect posture, and the blood should be drawn from a large aperture, so as to induce a fainting condition as rapidly as possibly, and care must be taken that the patient be not allowed to recover his heat and rapid circulation too soon, or the hemorrhage may recur. An early opportunity should be taken of administering one of those drugs which have an hæmostatic tendency (*e.g.*, plumbi acet., gr. j, in pil.; or acidi gallici, gr. v), in repeated doses. (p. 17.)

Wounds of joints.—Wounds of joints, if not obvious at first, are soon made evident by the escape of synovial fluid, which trickles out, and can easily be distinguished from blood by its light colour and tenacity. Closure of the wound is of the first importance, provided no foreign body is left in the synovial cavity. In the case of punctures or small incisions, collodion forms the best application, the same precautions being used as in cuts on the face. In the absence of collodion, white of egg is not a bad application; and plaister should be used to support the parts, and prevent the wound being dragged open. When

* The administration of the more powerful preparations of mercury, such as the iodide and the bichloride, is, we think, quite unnecessary in cases of infantile syphilis. The results of our own experience is in accordance with that of other practitioners. that the most severe cases may be both speedily and safely cured by hyd. c. cretâ and iodide of potassium, with a careful attention to the hygienic and dietetic means referred to in the text. Half a grain of hyd. c cretâ with one grain of iodide of potassium twice a day are, quite sufficient for an infant at the breast.—(ED.)

the wound is so large as to require the application of stitches, care should be taken not to insert them through the synovial membrane, and collodion may be advantageously applied over them. Perfect rest and the application of cold are the best preservatives against inflammation of the joint; and a splint should therefore be applied to the limb, which must be kept, if possible, in an elevated position. The posture which is easiest, and which relaxes all the parts most, must be the best at first; but, should inflammation come on, care must be taken to place the limb in a position in which it may ultimately be useful, should the motion in the joint be lost or impaired. Irrigation with cold water is the readiest and most certain method of applying cold to a wounded joint. (p. 37.)

Scalds of the glottis require special notice and treatment. The patient is generally a child who has attempted to drink boiling water from the spout of the tea-kettle, and has consequently injured the mouth, fauces, and upper part of larynx, more or less severely. The symptoms of dyspnœa will vary according to the time which has elapsed since the accident, and the amount of damage done. If the little patient is evidently *in articulo* from want of breath, the house-surgeon had better perform tracheotomy at once; but if the symptoms are not of such extreme urgency, the operation should be deferred, for a time at least, in order to try the effects of treatment. The first thing is to place the patient in a warm and moist atmosphere, and this can be contrived by placing an ordinary surgical cradle over him, and under the bedclothes or a piece of Macintosh cloth, and then bringing the steam of a kettle beneath the canopy thus formed. Care must of course be taken that the temperature be not raised above 80°, or the patient will be suffocated and parboiled. Relief may also be attempted by scarifying the back of the throat, epiglottis, &c., which has been known to be of great service in some cases.

As regards medicines, antimony and ipecacuanha appear to offer the best chance of success, and they may be most conveniently administered in the form of the wines. Large doses of either (and antimony by preference), according to the child's age, may be given, and frequently repeated, until the breathing is relieved. Vomiting is not to be wished for, and will seldom be produced. Mercury may be combined with the antimony, and to be of any service must be administered in heroic doses and frequently; but should the breathing become more embarrassed, the operation of tracheotomy must at once be had recourse to. (p. 45.)

Chalk-and-gum bandage is applied in exactly the same way as the starch bandage. The adhesive mixture is made by adding boiling water to equal parts of gum arabic and precipitated chalk; and this material has the advantages over the starch, both of becoming firm sooner, and of having more strength, so that the addition of strips of pasteboard is rarely necessary. (p. 150.)

[The work also includes the minor operations of surgery, clearly and concisely given; the dressing of wounds, the diagnosis and treatment of fractures and dislocations (chiefly the details omitted necessarily in systematic works); and concludes with a few pharmacopœial formulæ and diet tables from the London hospitals.]

162.—*Operative Surgery adapted to the Living or Dead Subject.*—by C. F. MAUNDER, F.R.C.S., Demonstrator of Operative Surgery. London Hospital.—*London*, Churchill, 1861.

[This useful little work is contained in two small volumes. It will chiefly prove of service to students practising operations upon the dead body. The following short extract on a method for perineal section, as practised by Mr. Cock, is new to us.]

When attempting to open the urethra in the perineum, the subject should be placed in the lithotomy position. The patient being so placed, the operator's left forefinger, well oiled, should be passed into the rectum, to ascertain the exact position of the apex of the prostate, and be there maintained; with the right hand a double-edged scalpel incises the median line of the perineum anterior to the anus, the point of the knife being carried back, with the intention of opening the membranous urethra in the direction of the apex of the prostate, to which latter the finger in the rectum serves somewhat as a guide. As soon as the knife has entered the perineum, it should not be withdrawn till the requisite section of parts is completed.

This mode of treatment is very applicable to cases in which extravasation of urine has occurred behind a stricture. A ready means of escape is thus offered, both to the urine already poured out into the surrounding parts and also to that afterwards secreted. While the perineal wound is patent, the structure may be treated by the usual method. (p. 156.)

163.—*Ready Rules for Operations in Surgery.* By ALLEN WEBB, M.D., F.R.C.S.L., Presidency Surgeon, Surgeon Superintendent to the Native Hospital, Calcutta, &c. &c.—*London*, Churchill, 1861.

[Most surgeons when about to operate are particular to see that all the instruments and assistants are properly arranged. Dr. Webb's book is the best of the kind we have seen, and will greatly help to facilitate the arrangements previous to an operation. It ought to be in the possesion of every operating surgeon, and in every hospital. Two pages are devoted to each operation. The book being laid open, on the left page are the names of all the instruments required; and, printed in red letters, every thing that each assistant has to do, "the great aim being that the assistants should know what to do in concert

with the operator, at the very time that the surgeon requires it done." On the right page are directions for the operator himself; and space enough is left on each page to add any further directions which he may think necessary.]

164.—*The Composition of the Urine in Health and Disease.* By EDMUND A. PARKES, M.D.—*London*, Churchill, 1860. pp. 404.

[This is a most complete work on the urine; the whole subject is thoroughly and practically considered. The first part of the work treats of the urine in health, and the variations caused in its composition and amount by every conceivable physiological condition, and also by the use of medicinal agents. The second part treats of the urine in disease, the abnormal constituents and urinary sediments; then the state of the urine in every separate disease is treated of in a most able and concise manner. We give one or two brief abstracts on points worth remembering.

In all congestive diseases of the liver, the absorption of water from the stomach and intestines is so much lessened in amount that a copious or even natural flow of urine is at once a strong argument against the existence of congestion or cirrhosis of the liver.

On the subject of diabetes, Dr. Parkes observes, that it is not absolutely proved that all the starch ingested by diabetics appears as sugar in the urine, but this is probable. In two patients, examined carefully for two months, the quantity never exceeded the amount of starch, and almost all the starch was accounted for by the diabetic sugar. In one case abstinence from starchy food completely freed the urine from sugar. In the later stages of the disease sugar is not, however, formed entirely from the starchy ingesta, but also from albuminous foods.

We think this work well suited for a book of reference; and to medical men in extensive practice it would prove very useful, as the contents are so arranged, that, without much trouble, the state of the urine in any particular disease may be referred to; also the action or effect of any internal remedy upon the urinary secretion.]

165.—*On Urine, Urinary Deposits and Calculi.* By LIONEL S. BEALE, M.B., F.R.S., Physician to King's College Hospital.—*London*, Churchill, 1860. pp. 424.

[Few books have afforded us more pleasure in perusal than this of Dr. Beale's. We give some extracts upon the most important parts, which will show the character of the work, and also we hope prove interesting to our readers. Speaking of the circumstances affecting the formation of urea, the author observes:]

In all probability, urea is formed in the organism by the oxidation of uric acid. The latter substance, if the oxidating processes in the body are active, becomes resolved in great measure into urea and carbonic acid; but if, on the other hand, these processes are less active than they should be, the uric acid in the urine is increased in quantity; a certain quantity of oxalic acid, and other substances of a lower degree of oxidation than urea, seem to be produced, instead of nearly the whole of the uric acid being resolved into this soluble substance. Wohler and Frerichs have shown that, if uric acid be taken at night, oxalate of lime is found in the morning urine; and Neubauer found that, when rabbits were made to take a considerable quantity of uric acid with their food, the urea in their urine increased from 1·34 to 4 grammes (from 20·67 to 61·72 grains). Large quantities of fluids cause an increase in the proportion of urea formed in the organism. A dilute state of the solids, is favourable to their oxidation; and in certain conditions, where these changes are but imperfectly carried on —and in consequence uric acid accumulates in the blood, or at most is resolved into oxalic acid—the further oxidation is promoted by the administration of increased quantity of fluid, especially of fluids containing alkalies, which not only increase the activity of the changes, but effect the solution of the insoluble uric acid and urates. Hence the benefit of alkaline waters, baths, moderate exercise, and plenty of good air, in gout and other conditions in which much more uric acid is formed than can be, under ordinary circumstances, converted into urea.

The quantity of urea excreted is also increased by commonsalt. (Bischoff). It is probable that not only does chloride of sodium, so to say, filter through the different tissues, like other saline substances, and thus drive out certain materials which are contained in their interstices; but that it also facilitates the occurrence of chemical change in the body, and directly influences the quantity of urea formed. The importance of chloride of sodium to cell-growth and the development of different textures, and its value in nutrition, are well known. (On the Chlorides in Pneumonia, *Med.-Chir. Trans.*, vol. xxxv.) The beneficial effect of alkalies in different cases is generally acknowledged; and it is probable that this is in part to be explained by the influence they have been proved to possess in promoting chemical change in the body, and especially in favouring the oxidation of albuminous substances. (p. 24.)

On the other hand both the solids and fluids of the urine are diminished by alcohol; so also is the proportion of carbonic acid exhaled. Tea causes a diminution both in the quantity of urine and fæces, as the beautiful researches of Dr. Böcker have conclusively proved. (*Medico Chirurgical Review*, vol. xiv.) Coffee exerts a similar effect, which seems to be due, not to the caffein, but the empyreumatic oil which it contains, according to Julius Lehmann. These substances, tea, coffee, and alcohol, in moderate quantity, affect the disintegration of tissue, and directly diminish the quantity of the

excrementitious substances formed in the process. Supposing the food to be insufficient, the loss of weight which must necessarily take place in the body would be lessened; and they may therefore be regarded as advantageous, not only in economising the food, but in limiting to some extent the waste of the albuminous tissues. (p. 25.)

[The following extract is from a paragraph treating of increased acidity of the urine:]

A highly acid condition of the urine, persisting for a long period of time, may cause the precipitation of uric acid, and so lead to the formation of a calculus. Acid urine not unfrequently causes irritable bladder, and excites other morbid actions. In most cases, the salts of the vegetable acids (citrates, acetates, tartrates) will be found more efficient in counteracting this acid state of the urine, than alkalies or their carbonates, and are less likely to interfere with the digestive process. There are, however, low conditions of the system in which the acid state of the urine, and a tendency to the deposition of uric acid in large quantity, are not relieved by alkalies; on the contrary, such cases are often much benefited by an opposite plan of treatment —tonics and the mineral acids before meals, a nourishing diet, with a moderate supply of simple stimulants with a little alkali, or with alkaline waters. Pepsine is often of great use in these cases. Many of them seem to be intimately connected with impaired digestive power. The acid state of the urine may then depend upon very different conditions of the system, and these must be carefully considered in each individual case before any plan of treatment is suggested. (p. 82.)

[There is a class of cases, in which the urine contains a positive daily excess of urea. The symptoms are as follow:]

The patient is weak, and grows thin, in spite of taking a considerable quantity of the most nutritious food. He feels languid, and indisposed to take active exercise. In some cases, digestion is impaired; in others, the patient eats well, experiences no pain or uneasiness after food, and perhaps has a good appetite. Sometimes there is lumbar pain. It would seem that much of the albuminous material in the blood, instead of being applied to the nutrition of the tissues, becomes too rapidly converted into urea, and is excreted. The waste of the tissues is not properly repaired, and the patient gets very thin. To refer these symptoms to the existence of a particular diathesis, appears to me no explanation of the nature of the case. The pathology of these remarkable cases has not yet been satisfactorily investigated. Mineral acids, rest, shower-baths, and good air, often do good; but some of these patients are not in the least benefited by remedies, and they continue for years very thin, passing large quantities of highly concentrated urine, while the appetite remains good, and they digest a considerable quantity of nitrogenous foods. I am now trying, in one of these cases, which has resisted the usual plans of

treatment, the effect of pepsine, with diminished quantity of meat, and a larger amount of farinaceous food. The condition often lasts for some years, and then the patient's health improves, and he gets quite well. (p. 90.)

[The following will show Dr. Beale's opinion of what is usually called the oxalic acid diathesis.]

We shall see that oxalate is one of the commonest urinary deposits; that it results from decomposition; that there is no reason for believing it to be indicative of any peculiar diathesis or habit of body. (p. 91.)

[The occurrence of pus in the urine must always prove of much interest in a clinical point of view. In one case it may be of no importance, in another it may show serious disease, and every medical man ought to be able to form some opinion whence the pus is derived and what it indicates. In the female, small quantities of pus may be derived from the mucous membrane of the vagina, and may be of little or no importance. Pus may come from any part of the genito-urinary mucous membrane, or from an abscess opening upon any part of its surface.]

It is often difficult to form an opinion as to the exact seat of formation of the pus; and it must be obvious that we ought never to come to a decision on such a point until we have accurately weighed all the evidence that a careful investigation of the case will afford. Microscopical examination will give us important help; but we must not rely upon this, or indeed upon any single mode of investigation. The question is an extensive one, and I shall only refer to one or two points connected with the evidence deduced from microscopical examination, as you are quite familiar with the nature of the other evidence to be obtained in such cases. You may often form some idea of the locality from which the pus has been derived by examining attentively the characters of any epithelial cells which may be mixed with it. (Lecture IX.) When pus is derived from the bladder, it generally contains crystals of triple phosphate, and granules or small spherules of earthy phosphate mixed with it; and the symptoms of the case will generally enable you to decide if the pus is formed in this viscus. Large quantities of pus may escape from the bladder for a number of years. I know of one gentleman who has passed pus in considerable quantity from the bladder during a period of twenty-five years. The suppuration of the bladder may be due to gonorrhœa, to gout, and to a state of mucous membrane which is termed catarrh of the bladder. When the pelvis of the kidney is dilated and sacculated (a form of pyelitis), the quantity of pus passed in the urine is often enormous; and this may last for years, until the kidney becomes a mere pus-forming cyst, which, in favourable cases, gradually contracts; the formation of pus ceases; the cyst slowly wastes; and the patient

perfectly recovers; the work of the two kidneys being performed by the remaining one, which has gradually undergone an increase in size corresponding to the increased work it has been called upon to perform. When pus is derived from the pelvis of the kidney, the earthy phosphates are often completely absent. There is a chronic state of ulceration of the ureters and pelvis of the kidney and bladder, in which pus is formed in considerable quantity, leading to the most distressing symptoms. Pus may depend upon the existence of old stricture.

Abscesses form in the kidney as in other organs; and, after the abscess has burst, pus makes its way into the urine. The inflammation of the mucous membrane of the kidney often extends upwards from the bladder. The presence of a calculus in the kidney, in the ureter, or in the bladder, may set up inflammation which may go on to the formation of pus. A very small calculus will sometimes excite great irritation in the kidney, so that both pus and blood are voided in the urine. (p. 281.)

[The importance of increasing the quantity of fluid taken by persons who suffer from certain varieties of urinary deposits must suggest itself to every one, but, from its simplicity perhaps, it rarely receives that amount of attention in the treatment of disease which it deserves.]

It is doubtful if that abundant deposition of urate of soda which is from time to time met with in almost all parts of the body, in certain cases, would have occurred at all, if the fluids had been constantly maintained in a proper state of dilution; and when these crystals have been deposited, we endeavour to remove them, or prevent further deposition, by diluting the fluids of the body, and by endeavouring to increase the solubility of the urate. We are perhaps too apt, in many chronic cases, to put patients upon a plan of treatment for so short a time as a few days or weeks; and our patients are often unreasonable enough to expect that remedies will remove in a week, matter which has been slowly accumulating perhaps for years. It is chronic cases of this kind which receive such real benefit from the comparatively prolonged course to which they are subjected in a German bath or hydropathic establishment; and it too often happens that, in endeavouring to perform quickly by remedies that which it is only possible to effect by giving large quantities of fluid during a considerable period of time, we disappoint ourselves and our patients; and perhaps in the end they attribute to some quack remedy or system, to which they have subsequently had recourse, a favourable result which is really due to the water they have drank and the hygienic rules to which they have been subjected, instead of to the nostrums they have swallowed.

In certain cases of gout, in chronic rheumatism, and in many cases where uric acid and urates are constantly deposited in the urine or in the tissues of the body, the most important of all things is to ensure

the thorough washing out of the system. Exercise when it can be taken, hot baths, Turkish baths, &c., by promoting sweating, excite thirst; and thus more fluid is ingested, which is soon got rid of by various emunctories, carrying out with it insoluble substances, the fluid removed being soon replaced by a fresh quantity. In the frequent repetition of these processes from time to time, a vast quantity of fluid is made to pass through the body, with the most beneficial effect.

You would be surprised how very little fluid some persons take, as a rule; and this fluid, small as it is, is often saturated with soluble substances. The fluid thus introduced is, in many persons who live well, barely sufficient to hold the various compounds in solution while undergoing chemical change. Many dislike to drink water, and not a few have a strong prejudice against it; and these are often the very individuals whom we find suffering from gout, rheumatic pains in the muscular and fibrous tissues, and various forms of urinary deposits. They will receive the greatest benefit from moderate sweating, and alkalies dissolved in a large quantity of water. You will seldom find difficulty in prevailing on patients to take seltzer, vichy, or other alkaline waters daily, although it would be useless to recommend them to take pure water. They can take them with their wine at dinner, the last thing at night, and perhaps the first thing in the morning. People who live well, or rather too well, will soon find out that they must continue this plan, and take now and then small doses of alkalies. It is quite superfluous for me to enter into the minute details applicable in individual cases; but I cannot too strongly recommend you to study very carefully the different symptoms in cases of this class; for I feel sure that much permanent relief may be afforded such patients by explaining to them the importance of constantly attending to simple rules based on the principles to which I have adverted. Although I may have been somewhat tedious, I feel sure that you will not think that I have dwelt longer on these points than their practical importance demands; and I am convinced that we shall practice our profession with greater advantage to our patients, and advance its interests more, by studying carefully the nature of the actual processes going on in disease, and considering how these processes are to be modified by simple means and a few remedies whose action is certain and well understood than by hunting for new specific medicines, or combining together a great number of compounds, many of which are completely modified as soon as they enter the stomach, and are certainly destroyed long ere they reach the part of the organism where we desire that they should exert their specific influence. (p. 356.)

[It is our belief that scientific medicine in its present state will make more advance by the study of animal chemistry than by any other means, and we always view with pleasure such a work as this of Dr. Beale's, which is pleasant to read and full of instruction on subjects of practical importance.]

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